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ORIGINAL ARTICLES.

I.—Restoration of *Camptosaurus*.

By Prof. O. C. Marsh, Ph.D., LL.D., F.G.S., etc.

(PLATE VI.)

THE Jurassic deposits of Western North America contain the remains of many gigantic Dinosaurs, and various skeletons of these have been obtained by the writer, who has described the more important forms. Restorations of the skeletons of three of the most interesting genera, *Brontosaurus*, *Stegosaurus*, and *Ceratosaurus*, have already been given in the American Journal of Science and in this Magazine, and another of these huge reptiles is thus represented on Plate VI. accompanying the present article. Each of the three forms previously restored was a typical member of a distinct group of the *Dinosauria*, and this is true, although in a less degree, of the present genus, *Camptosaurus*. Restorations of *Anchisaurus* from the Triassic, and *Claosaurus* and *Triceratops* from the Cretaceous, all Dinosaurs of much interest, have likewise been published by the writer in the American Journal[1] and in this Magazine.

The restoration here given is based upon the type specimen of *Camptosaurus dispar*, one of the most characteristic forms of the great group *Ornithopoda*, or bird-footed Dinosaurs. The reptile is represented on Plate VI., one-thirtieth natural size. The position chosen was determined after a careful study not only of the type specimen, but of several others, in excellent preservation, belonging to the same species or to others nearly allied. It is therefore believed to be a position frequently assumed by the animal during life, and thus, in some measure, characteristic of the genus *Camptosaurus*. The present species, when alive, was about twenty feet in length, and ten feet high in the position here represented.

The genus *Camptosaurus* is a near ally of *Iguanodon* of Europe, and may be considered its American representative. *Camptosaurus*, however, is a more generalized type, as might be expected from its lower geological horizon. It resembles more nearly some of the Jurassic forms in England, generally referred to *Iguanodon*, but, as these are known only from fragmentary specimens, their generic relations with *Camptosaurus* cannot now be determined with certainty.

[1] American Journal, vol. xli. p. 339, April, 1891; vol. xlii. p. 179, August, 1891; vol. xliv. p. 343, October, 1892; and vol. xlv. p. 169, February, 1893.

From the Upper Jurassic (Atlantosaurus beds) of Wyoming.

In comparing *Camptosaurus*, as here restored, with a very perfect skeleton of *Iguanodon* from Belgium, as described and figured, various points of difference as well as of resemblance may be noticed. The skull of *Camptosaurus* had a sharp, pointed beak, evidently encased during life in a horny sheath. This was met below by a similar covering, which enclosed the predentary bone. The entire front of the upper and lower jaws was thus edentulous, as in *Iguanodon*, but of different shape. The teeth of the two genera are of similar form, and were implanted in like manner in the maxillary and dentary bones. In *Camptosaurus* there is over each orbit a single supra-orbital bone, curving outward and backward, with a free extremity, as in the existing Monitor; a feature not before observed in any other Dinosaur except *Laosaurus*, an allied genus, also from the Jurassic of America. Other portions of the skull of *Camptosaurus* as well as the hyoid bones appear to agree in general with those of *Iguanodon*.

The vertebræ of *Camptosaurus* are similar in many respects to those of *Iguanodon*, but differ in some important features. In the posterior dorsal region, the transverse processes support both the head and tubercle of the rib, the head resting on a step, as in existing crocodiles. The five sacral vertebræ, moreover, are not coössified, even in forms apparently adult, and to this character the name *Camptonotus* first given to the genus by the writer in 1879 especially refers.[1]

Another notable feature of the sacral vetebræ of the type specimen should be mentioned. The vertebræ of the sacrum, especially the posterior four, are joined to each other by a peculiar peg and notch articulation. The floor of the neural canal of each vertebra is extended forward into a pointed process (somewhat like an odontoid process), which fits into a corresponding cavity of the centrum in front. This arrangement, while permitting some motion between the individual vertebræ, helps to hold them in place, thus compensating in part for absence of ankylosis. A similar method of articulation is seen in the dermal scales of some ganoid fishes, but, so far as the writer is aware, nothing of the kind has been observed before in the union of vertebræ.

In *Camptosaurus* the sternum was apparently unossified, and no trace of clavicles has been found. The pelvis of *Camptosaurus* differs especially from that of *Iguanodon* in the pubis, the postpubic branch being even longer than the ischium, while in *Iguanodon* this element is much shortened.

In the fore foot of *Camptosaurus* there were five functional digits, the first being flexible and nearly parallel with the second, thus differing from the divergent, stiff thumb of *Iguanodon*. The hind feet had each three functional digits only, the first being rudimentary and the fifth entirely wanting, as shown in Plate VI. The entire skeleton of *Camptosaurus* was proportionately more slender and delicately formed than that of *Iguanodon*, although the

[1] This name proved to be preoccupied, and *Camptosaurus* was substituted for it. —American Journal, vol. xxix. p. 169, February, 1885.

habits and mode of life of these two herbivorous Dinosaurs were doubtless very similar.

The type specimen of *Camptosaurus dispar*, used as the basis of the present restoration, is from the Atlantosaurus beds of the Upper Jurassic of Wyoming. This species and other allied forms will be fully described in an illustrated Memoir now in preparation by the writer for the United States Geological Survey. The present restoration is reduced from a large drawing made for that volume.

II.—On the Structure and Affinities of the Genus *Solenopora*, together with Descriptions of New Species.

By Alex. Brown, M.B., M.A., B.Sc.;
Assistant in the Natural History Department in the University of Aberdeen.

(Part II.)

(*Continued from the April Number*, p. 151.)

5. *Solenopora filiformis*, Nich.

Solenopora filiformis, Nicholson, Geol. Mag. 1888, Dec. III. Vol. V. No. 1, p. 21.

"It presents itself sometimes in the form of small rounded masses, or irregular nodules; or at other times as lobate or ramified masses of considerable dimensions. Viewed with a powerful magnifying-glass, it appears to be quite compact or obscurely fibrous" (Nicholson).

The internal structure is commonly much obscured, or even destroyed by crystallization. In vertical section the cells are arranged in radiating fashion. Cells narrower and shorter than in *Solenopora compacta*, diameter being $\frac{1}{24}$ mm. Cell-walls thin and not wavy (Fig. 6A). In tangential section the cells have

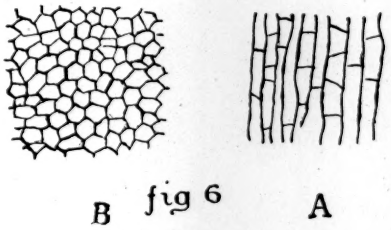


Fig. 6.—A, Long. sect. of *Solenopora filiformis*, Nich., from the Ordovician Limestones of Craighead, Girvan, Ayrshire. × 60 d. B, Tang. sect. of the same.

polygonal walls. They appear to increase by ordinary cell-division, but the active condition has not been observed (Fig. 6 B).

Observations.—This species seems to be an intermediate form between *S. compacta* and *S. Jurassica*. The cells are not so elongated as in *S. compacta*, and are more like what one finds in *S. Jurassica*; only in the latter the transverse cell-walls are concave

on one side, and are as thick as the longitudinal walls, while in *S. filiformis* the transverse cell-walls are straight, and are thinner than the longitudinal walls. Further, in cross section, the cell-walls of *S. filiformis* are polygonal in form, while the same thing is found to occur very frequently in *S. Jurassica*.

Formation and Locality. — Ordovician, Craighead Limestones, Girvan, Ayrshire; occurring along with *Solenopora compacta*. (Coll. H. A. Nicholson.)

6. *Solenopora dendriformis*, n.sp.

Under this name I shall describe a species which is found occurring along with *Solenopora compacta* and *Solenopora nigra* in the limestones of Saak, Esthonia. It presents itself in minute nodules, ovate or spherical in form and dark in colour. Each nodule shows the arrangement of its parts in concentric layers.

In longitudinal section the cells appear to have a radial arrangement; cell-walls thick and sinuous; transverse walls concave on their peripheral side. The diameter of the cells in each specimen varies very greatly, this being due to the peculiar branching nature of the cells to be presently referred to. The largest diameter is about $\frac{1}{17}$ mm., but others are very much smaller. The length of the cells is about $\frac{1}{5}$—$\frac{1}{3}$ mm. or even longer.

In tangential section the cells appear to branch in all directions, especially horizontally (Fig. 7). The smaller branching processes are seen cut across in the illustration.

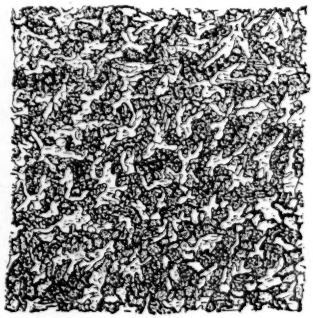


fig 7

Fig. 7.—Tang. sect. of *Solenopora dendriformis*, n.sp., from Esthonia. × 50 d.

Observations.—The important feature in this species is the extraordinary nature of the cells, branching as they do in all directions. Otherwise the nature of the skeleton is much the same as in the species already described. The position and form of its transverse cell-walls show a resemblance to what we find in *S. Jurassica* and *S. nigra*.

Formation and Locality.—Ordovician Limestones of Saak, Esthonia. (Coll. H. A. Nicholson.)

7. *Solenopora fusiformis*, n.sp. (Pl. V. Fig. 4.)

This species occurs as small white nodules, oval, ovate, or spherical in form. Concentric rings faintly seen. Its size varies from the dimensions of a pea to those of a hazel-nut. The structure is generally greatly obscured by crystallization.

In longitudinal section numerous concentric rings are seen, but many of them are altered by secondary crystallization. The cells have about the same diameter as the cells of *Solenopora filiformis*, viz. $\frac{1}{2\frac{1}{4}}$ mm., but are more or less spindle-shaped, and dove-tail into one another.

In tangential section the outline of the cells is only feebly sinuous. The cells vary in size considerably on account of their fusiform nature.

Observations.—The interesting feature here is the fusiform nature of the cells, which can be more readily distinguished under a high power. In this type, the concentric rings are particularly well developed, and the general structure reminds one of such a form as *Lithothamnion.*

Formation and Locality. — Ordovician, Craighead Limestones, Girvan, Ayrshire, occurring in great quantity, and forming large masses of limestone. It is associated with *S. compacta* and *S. filiformis.* (Coll. H. A. Nicholson.)

II. Relations of the Genus *Solenopora.*

The observations previously recorded render it possible to arrive now at something like a definite conclusion as to the affinities and systematic position of the genus *Solenopora.*

In the Tertiary rocks, and among existing Algæ, we find the well known Nullipores, many of which are massive in structure. Their distribution is very wide, recent forms occurring as far north as the Arctic region. They form extensive masses of limestone, as the Tertiary era bears witness. To this group of Coralline Algæ the forms above described, as helping to give rise to thick masses of limestone during the Ordovician and Jurassic periods, appear to be closely related.

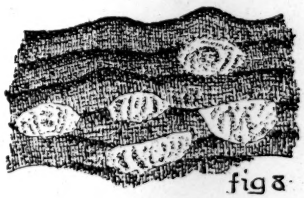


Fig. 8.—Long. sect. of *Lithothamnion fœcundum.* × 40 d. (after Kjellman).

In Fig. 8 and Pl. V. Fig. 1, the general structure of a Nullipore (*Lithothamnion*) is seen. It consists simply of a mass of cellular tissue arranged in concentric rings, which rings are often irregular. The cells are, in section, quadrilateral in shape, and are of very

small size. In some specimens spaces can be seen which are the remains of the former conceptacles of sporocarps or sporangia. These cavities communicate with the exterior by means of a number of canals. The latter are generally surrounded by a number of cells having a different shape from the ordinary tissue-cells of the thallus. This is specially well seen at the external apertures of the canals (Fig. 9), where the modified cells

Fig. 9.—Part of roof of "conceptacle" decalcified. × 40 d. (after Kjellman).

in question give rise to a rosette-like appearance. These conceptacles may or may not be immersed in the general substance of the thallus. If immersed, then a scar or a vacant space is left to tell of their existence; or, indeed, they may be replaced by new tissue, so that no trace of the former conceptacle can be seen.

Regarding other forms of the Corallineæ, it is necessary to mention one or two points for comparison. In *Corallina mediterranea* there is no concentric arrangement of the tissue-cells, as there is in *Lithothamnion*. The cells, too, are larger than in the latter, and

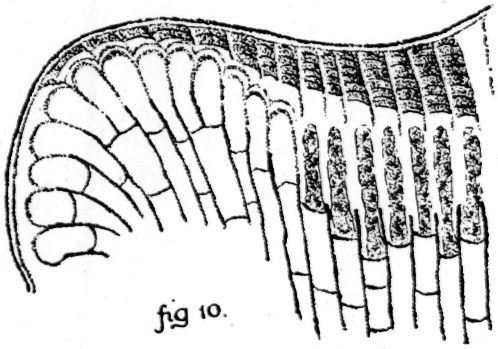


Fig. 10.—Long. sect. of the apical shoot of *Corallina mediterranea*. × 300 d. (after Solms-Laubach).

are arranged as in that genus in parallel rows, save where some cell in one of the rows has two cells attached to its distal extremity in a sub-dichotomous manner (Fig. 10). The last feature is, however, characteristic of certain extinct forms of *Lithothamnion*, according to A. Rothpletz (*op. cit. supra* p. 6).

In *Amphiroa exilis* and *Amphiroa rigida* the concentric arrangement

of cells is excellently seen (Fig. 11). The cells are fairly large, and occupy the whole space between each dark concentric line.

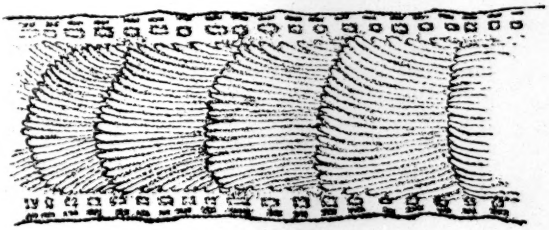


fig. 11

Fig. 11.—Long. sect. of thallus of *Amphiroa exilis*. × 160 d. (modified after Solms-Laubach).

In another form, *Melobesia deformans*, it is necessary to note the arrangement of the spore-bearing cells (sporangia) in relation to the tissue-cells of the thallus. The illustration in Fig. 12 will clearly show this, where the larger cells are tetraspore-producing cells, and the smaller the ordinary tissue-cells of the thallus.

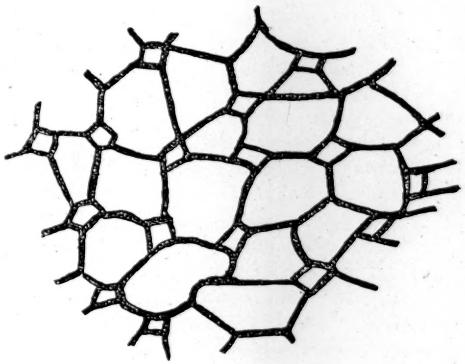


fig. 12

Fig. 12.—Surface view of the thallus of *Melobesia deformans*, showing tetrasporangia. × 450 d. (after Solms-Laubach).

Let us now examine the points wherein the ancient form *Solenopora* resembles members of the group of calcareous Algæ.

(*a.*) *Structure, Growth, and Arrangement of tissue-cells.*

In existing forms of *Corallineæ* the concentric arrangement of cells, though very general, is not always observed. As regards

Solenopora, it is most distinct in some specimens of *S. compacta* (Fig. 1), *S. lithothamnioides* (Pl. V. Fig. 2), and *S. fusiformis* (Pl. V. Fig. 4), in which species the arrangement is quite comparable to what obtains in *Amphiroa* (Fig. 11). To the naked eye mostly all these species appear to have these concentric rings. Under the microscope, however, this appearance, in many cases, is not so distinct, and the dark rings do not correspond with the junction between neighbouring concentric rows of cells; but there is a thickening over a wider area (see Pl. V. Fig. 3). This is really the condition we observe in specimens of Nullipores (*cf.* Pl. V. Fig. 1 and Fig. 8). In some cases the cause of the concentric markings is partly due to the concentric areas of secondary crystallization (*e.g.* in *Solenopora Jurassica*). Again, the appearance and structure of the cell-walls are very similar in *Solenopora Jurassica* and *Corallina mediterranea*. In both, the transverse cell-walls are concave on the peripheral side (Figs. 4 and 10). In the case of *Solenopora lithothamnioides* and *Lithothamnion*, each concentric layer is composed of numerous cells arranged in parallel rows (*cf.* Pl. V. Figs. 1 and 2), the main difference between the two forms consisting in the fact that the cell-walls of the latter are thickened over a wider area, whereas those of the former are thickened only along each of the concentric lines. In *Solenopora compacta* we find the sub-dichotomous and sub-trichotomous origin of peripheral cells, referred to in the first part of my paper. This method of increase is characteristic of *Corallina mediterranea* and *Lithothamnion* (*cf.* Figs. 1 and 10). It probably also occurs in *Solenopora Jurassica* and *Solenopora nigra*. In all the forms mentioned, both fossil and recent, increase in number of cells is more common by longitudinal than by transverse fission. It is pointed out by A. Rothpletz (*op. cit. supra* p. 6) that longitudinal fission is quite characteristic of certain extinct forms of *Lithothamnion*, and that in these this is the ordinary method of growth. Thus we have a distinct likeness in the mode of cell-division between the genus *Solenopora* and the calcareous Algæ. Lastly, account must be taken of the form of cells found in *S. fusiformis* and in *S. dendriformis*. In the former the cells lying in each concentric layer have a fusiform appearance, and to a greater or less extent dovetail into each other (Pl. V. Fig. 4). This condition is of very common occurrence in the vegetable kingdom; and, with the next point to be mentioned, affords a strong proof of the cellular nature of the genus. In *S. dendriformis* the cells in cross section show a peculiar branching condition, mostly in a horizontal direction, the branches filling up all the interspaces between the bodies of the cells. This structure forcibly reminds one of what is characteristic of certain Algæ, in which certain cells of the thallus assume a branching habit, *e.g.* in *Fucus*.

Thus the forms of the cells and cell-walls, the method of increase, and the arrangement of the tissue-cells in the various species of *Solenopora* bear strong evidence of relationship between that genus and the calcareous Algæ.

(b.) Size of the cells of Solenopora.

It may be objected that since the cells of *Solenopora* are so much larger than those of a Nullipore, the genus *Solenopora* can hardly be compared with the recent calcareous Algæ. A comparison between the cells of various genera of the Corallineæ and the cells of the genus *Solenopora*, will dispel all such objections. A glance at Fig. 13 shows the marked variations in the size of the cells of a recent Nullipore (A), of *Amphiroa exilis* (B), of *Solenopora filiformis* (C), and of *Solenopora Jurassica* (D), all being drawn to the same scale.

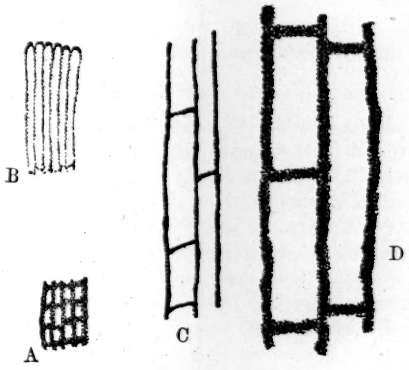


fig 13

FIG. 13.—A Cells of a recent Nullipore (*Lithothamnion*). × 160 d.
B Cells of *Amphiroa exilis*. × 160 d. (modified after Solms-Laubach).
C Cells of *Solenopora filiformis*, Nich. × 160 d.
D Cells of *Solenopora Jurassica*, Nich. × 160 d.

Such an illustration at once clears up any difficulty on the above point, for there appear to exist all gradations of size of the tissue-cells.

(c.) Reproductive organs.

Evidence in regard to these is not yet so complete as one would wish; still, examination of some specimens clearly shows traces of structures that can be accounted for in no other way than by regarding them as the remains of reproductive organs.

Thus, a specimen of *Solenopora compacta*, var. *Trentonensis*, on examination of a tangential section, is seen to possess two distinct kinds of cells, as mentioned above in the description of this species. The larger cells here (Fig. 2) are sporangia, and can be compared with the sporangia of *Melobesia deformans* (Fig. 12), where they exist as large cells among the ordinary tissue-cells of the thallus.

Again, a tangential section of a specimen of *Solenopora Jurassica*, shows certain spaces amongst the general cellular mass. These spaces are oval or circular in form, and in one specimen from Malton, Yorkshire, are arranged in a concentric row. The latter

specimen, however, was so much destroyed, that one could not, with certainty, decide as to the true state of matters. Further examination of such specimens will clear up all doubt as to the existence of these spaces. In the same sections are seen clusters of cells at intervals, arranged in rosette-like fashion (Fig. 5). One is impressed with the striking similarity all this bears to the conceptacle-spaces and the canals of *Lithothamnion* (Figs. 8 and 9), the rosette-cells of *Solenopora* representing the similarly arranged cells around those canals. It is, therefore, highly probable that the spaces observed in *S. Jurassica* are really the remains of once existing conceptacles, and that the rosette-cells surrounded the passages leading into them. It will, however, require further and more extended observation to confirm such conditions as these.

(*d.*) *Modes of Occurrence.*

It may be objected that these forms, here referred to the genus *Solenopora*, are not in fact generically identical. Such an objection, however, must fall to the ground, for it is impossible to overlook the fundamental likeness which subsists among them all. They are found under the same conditions, associated in the same kind of rocks, and taking part in the formation of the same great masses of limestone, whether they belong to the Ordovician, the Silurian, or the Jurassic systems. They have, too, the same external characteristics, having the appearance of pebbles or concretions, with a lobulated or subspherical outline. On fracture the surfaces of all present a porcellanous or fibrous aspect, and exhibit the characteristic concentric structure. Many of the *Solenopora* limestones show scarcely any differences in outward and internal appearance from the ordinary Nullipore limestones; and, indeed, it is often very difficult to distinguish between them.

(*e.*) *Difficulties in retaining the genus Solenopora in the Animal Kingdom.*

In assigning the genus *Solenopora* to a definite place in the scale of life, it seems clear that, if an animal at all, it must be referred to some group or other of the Cœlenterates. But no recent Cœlenterate has zoöidal tubes as minute, or nearly as minute, as are those of *Solenopora*. This is the great difficulty in assigning to the genus a place in the Animal Kingdom. Still more, were it of animal origin, would it be difficult to account for the largely branched cells of *Solenopora dendriformis*, the fusiform cells of *Solenopora fusiformis*, and the essentially *cellular* nature of the skeleton in well-preserved examples. Taking all the evidence as we now have it, we may reasonably conclude that the genus cannot be referred to the *Actinozoa*; and "we cannot refer it to the *Hydrozoa*, for we are not acquainted with any *Hydrozoön*, living or extinct, with which *Solenopora* could be compared. It shows no features in its minute structure which remind us of the Hydrocorallines, and it assuredly presents no structural resemblance to any known type of the Stromatoporoids" (Nicholson).

On the other hand, I have been able to determine with more or less certainty that the structure of *Solenopora* is truly *cellular* and not *tubular*, a determination which Prof. Nicholson clearly foresaw as the possible result of further investigation. I have shown that the structure of the cells and cell-walls and the mode of growth are, in certain cases, exceedingly similar to those of certain recent and extinct Corallineæ; and I have further shown that the comparatively large size of the cells in *Solenopora* need not necessarily be a barrier to our regarding the genus as truly referable to the calcareous Algæ. Lastly, I have been able to demonstrate in some forms of *Solenopora* the existence of structures, which in all essential respects may be fairly compared, some with the "tetrasporangia," and others with the "conceptacles" of recent Corallineæ.

Upon the whole, therefore, the balance of evidence seems to be unequivocally in favour of the reference of the genus *Solenopora* to the series of the Nullipores. Should this view be finally established, it will be an interesting proof of the extent to which the calcareous Algæ have taken part in the formation of the older of the Palæozoic limestones, as they are already known to have done in the case of some of the Tertiary and Secondary limestones. This proof will be still further enforced if, as seems extremely probable, it should ultimately be shown that *Girvanella*, Nich. and Eth., is really referable to the *Siphoneæ verticillatæ*.

III.—On the Results of Unsymmetrical Cooling and Redistribution of Temperature in a Shrinking Globe as applied to the Origin of Mountain Ranges.[1]

By T. Mellard Reade, C.E., F.G.S., etc.

HAVING been honoured by an invitation to state to your Society the physical principles upon which my Theory of the Origin of Mountain Ranges is founded, I do so with the greater pleasure knowing that it will receive a searching criticism at your hands, from which I hope to derive considerable benefit. My audience hitherto having been mostly geological, the physical principles involved have been to some extent subordinated to geological conditions.

In addressing you I purpose adopting the opposite method to that in which I have expounded the process of Mountain formation, as I conceive it to have taken place, to my geological brethren. In the natural way in which the subject has developed itself in my own mind, I commenced with the facts of geology and reasoned backwards to the principles. On the present occasion I intend to state a few of the leading physical principles, and to unfold how the facts of Geology, of Mountain Upheaval and Structure have come about.

[1] Read before the Liverpool Physical Society, January 29th, 1894.

The Earth considered as a Homogeneous Cooling Solid.

Adopting Lord Kelvin's view as stated in Thomson and Tait's Natural Philosophy (1867, pp. 711–727), that since the beginning of Geological Time the Earth has been a highly heated solid body cooling into space by being kept at a constant lower temperature at the surface, we will try and trace out the physical consequences that must follow.[1] We will, for the purposes of investigation, first assume that the globe is homogeneous throughout and that the cooling and outward flow of heat is equable on all radii, or otherwise symmetrical. Lord Kelvin assumes that at the time of first solidification the augmentation of temperature downwards, or the temperature gradient, was 1° Fahr. in 10 feet, which has gradually decreased to 1° in 50 feet, this being taken as the average at the present time.

The time that has elapsed between these two conditions hardly forms an element in our present considerations; but Lord Kelvin, in the paper referred to, worked it out to 96,000,000 years. On p. 719 is a diagram, one curve on which shows the excess of temperature above that of the surface; the other shows the rate of augmentation of temperature downwards that has taken place in 100 million years of cooling. Lord Kelvin says: "Thus the rate of increase of temperature from the surface downwards would be sensibly $\frac{1}{51}$ of a degree per foot for the first 100,000 or so; below that depth the rate of increase per foot would begin to diminish sensibly. At 400,000 feet it would have diminished to about $\frac{1}{141}$ of a degree per foot; at 800,000 feet it would have diminished to less than $\frac{1}{50}$ of its initial value, that is to say to less than $\frac{1}{2500}$ of a degree per foot; and so on, rapidly diminishing as shown in the curve. Such is on the whole the most probable representation of the earth's present temperature at depths of from 100 feet, where the annual variations cease to be sensible, to 100 miles, below which the whole mass, or all except a nucleus cool from the beginning, is (whether liquid or solid) probably at or very nearly at the proper melting temperature for the pressure at such depth."

Level-of-No-Strain.

If these conclusions be just it is very obvious that not much more than a superficial shell of the earth has been affected by cooling. A hundred miles is but $\frac{1}{40}$ part of the earth's radius. Let us see how this affects the distribution of stresses and strains in a solid homogeneous globe.

It will need a little consideration to see that there are two elements involved—radial contraction and circumferential contraction. Both radial and circumferential contraction are, as we see, practically confined to a shell 150 miles thick (800,000 feet). Commencing very gradually from zero at the surface the contraction increases in intensity until it attains a maximum at about 50 miles, it then finally graduates into a practical zero at 150 miles deep.

[1] See also Sir W. Thomson, Brit. Assoc. Report, 1876, and Dr. C. H. Darwin, Phil. Trans. 1879, part i. It is considered that the earth is as rigid as steel, or at least glass, from the surface to the centre.

The circumferential rate of contraction of the sphere may be conceived as gradually increasing from zero at the surface in successive infinitesimal zones or shells, until the maximum contracting shell is attained, when the circumferential rate of contraction again diminishes, until practical zero is arrived at 150 miles below the surface.

In all these zones or shells the circumferential contraction is equal throughout for each particular shell. What I have called the circumferential contraction is equal throughout the circle, whereas the radial contraction differs at every point and is practically confined to $\frac{1}{25}$ of the radius.

What will happen is this: There will be one particular shell which will have the same rate of circumferential contraction as the average radial contraction taken from the point of intersection to the centre of the sphere. At this zone or surface there will be neither tension nor compression, no deformation in fact, and it is to this zone that the name of level-of-no-strain has been applied. Above the level-of-no-strain the shells will be in *compression*, beginning at zero at the level-of-no-strain and finishing at the surface with the maximum. Below, the shells will be in tension, beginning at zero at the level-of-no-strain, increasing to a maximum at about 50 miles deep, and then shading off to nothing, 150 miles below the surface.

This is a short statement of the principle I was the first to enunciate in chap. xi. of the "Origin of Mountain Ranges," published in 1886, in which I showed that the shell of compression could not be more than a few miles thick, or, to state it otherwise, that the neutral zone or level-of-no-strain between the shell in compression and the shell in tension could not be more than a few miles below the earth's surface.

I reproduce by the lantern the original diagram from the "Origin of Mountain Ranges." From this it follows that symmetrical secular cooling produces *tensile* strains in by far the larger bulk of the earth already affected by such cooling.

Previous to this demonstration it had been customary to look at the globe as consisting of a rigid crust in compression adapting itself to a shrinking nucleus without intermediate gradations. Reasoners desirous of explaining in this way the rugosities of the earth's surface, and the great evidences of lateral pressure shown by the folding of strata in mountain ranges, assumed whatever thickness they thought necessary for their theory, one well-known geologist even placing the crust in compression at 800 miles. The idea that any part of the shrinking globe was in tension did not seem to have occurred to any of the physicists or geologists who paid attention to the subject. I may add, as a matter of private history, that this conception, which had been floating about in my mind for a long time, was jotted down by me in a note five years before publication.

Since my work appeared, Mr. C. Davison contributed a paper to the Royal Society (read April 7th, 1887) entitled, "On the Distribution of Strain in the Earth's Crust resulting from Secular Cooling,"

in which he arrives at practically the same results as myself. He treats the problem mathematically, and locates the level-of-no-strain five miles below the surface after 174 million years of cooling. Attached to the paper is an Appendix by Prof. George Darwin, in which he calculates that after 100 million years of cooling the level-of-no-strain would be two miles deep. The Rev. Osmond Fisher has devoted a considerable amount of attention to the subject, and arrives at even a less result. I also showed that, whether we look at the earth's nucleus as solid or liquid, the level-of-no-strain would still be located in the hard shell or within the crust. Mr. Fisher has mathematically investigated the problem on the assumption that there is a liquid zone between a hard outward crust and a solid nucleus, and places the level-of-no-strain on this hypothesis of the condition of the earth's interior at four miles deep. Some people find a difficulty in taking in these conceptions. Perhaps it will help them to think of a ball of plastic clay drying. If it be dried slowly it parts with its moisture so equably that it becomes hard and solid without cracking. If, on the contrary, the drying is forced, it becomes fissured all over, because the surface portions contract more rapidly than the interior portions. This illustrates the contracting shell of the earth; but the contracting shell of the earth does not necessarily fissure, because of the weight of strata above, which keeps it solid by what I have called "compressive extension." The clay ball, however, gives no illustration of the shell of the earth, which is in compression because its matter does not gravitate towards its own centre but towards that of the earth.

Irregular condition of the Earth's Cooling.

The earth is, however, not an inert homogeneous ball, cooling equably into space, so it will be necessary to trace out the various modifying influences acting upon its surface. Chief of these is denudation and sedimentation. We may assume that in the planet's early history, volcanic action brought certain portions of the earth, either by upheaval or accumulation of ashes and lava, above the surface of the water as land. Then the land areas came under the joint influences of marine action and what is known as "subaerial denudation." The result was that large areas of the crust became covered with thick layers of sediment, lava flows, and ashes. What would be the effect of this padding upon the outflow of heat?

In 1834 Babbage, reasoning upon the causes which have produced the oscillations of level of the Temple of Jupiter Serapis, in the Bay of Baie, pointed out that the addition of sediment to any part of the earth's crust must raise the temperature of the portion of the crust it covers.[1]

[1] These consequences are worked out in a very ingenious manner, considering the time at which it was written, by Babbage in the 9th Bridgwater Treatise, in the Appendix Note F, pp. 182–201. Sir John Herschel's views, having a somewhat similar bearing, with the addition of what is now called Isostacy, are also given in a letter to Lyell, dated 1836, and published in Note 1, pp. 202–217.

I had not read these remarkably luminous sketches of a theory of elevation and subsidences until long after the publication of my "Origin of Mountain Ranges." The ideas are far in advance of the times, but are incomplete as not accounting for lateral pressure. Columnar expansion is the only thing considered.

Let us assume, as Lord Kelvin does, that the average rate of augmentation of temperature downwards is now 1° F. in 50 feet. In the early stages of the earth's geological history, as we have seen, it must have increased very much more quickly; but we will deal with the problem under present average conditions, which is stating the case in its worst aspect for my theory. Also, it will be convenient to assume that the sediments are being laid down in a shallow trough or basin of the ocean bordering a great continent, and that the waste of a very great area of land is being deposited off shore in waters of varying depths. It is a pretty well established fact that the area of deposition in such a case is considerably smaller than the area of denudation. If we take the waste of the continental land at a mean of 1 foot in 3000 years, the rate of deposition on the average will be as much more as the basin of deposition is smaller than the area of denudation, so if it be one-third, the mean rate of deposition will be 1 foot in each thousand years. I merely give these figures just to put the matter in somewhat like true perspective.

The sediment will all be laid down at an uniform temperature, say, of 50°; but as the matter accumulates a temperature grade will be established in course of time corresponding with the temperature grade due to the secular cooling of the earth as a whole; in other words, sediments, by obstructing the outward flow of heat, will augment the temperature downwards, so that the rocks of the ocean floor upon which the sediments rest will be at a temperature due to that depth.

If a mile of sediments have accumulated—and this is a mere trifle in the sedimentary crust of the globe—and the temperature gradient is 1° for every 50 feet, the rocks originally having a temperature of 50° F. will be raised to 155·6°, and this increase of temperature will eventually affect in a diminishing degree the whole of the rocks underlying the area of deposition. This effect is described in precise language by saying that the deposition of sediment raises the "isogeotherms" or isothermal surfaces in the underlying part of the earth's crust. We have assumed for the sake of simplicity, that the sediments deposited have the same mean conductivity as has been inferred for the whole of the earth's crust. It is possible that the mean conductivity of the sediments may be less, as experimental proof has been given that rocks are better conductors along the bedding planes than across them, and underlying rocks are often on edge.[1] If the sediments are worse conductors the increase of heat downwards would be more rapid, if better conductors the rate would be less.

It will now be seen that the laying down of sediment *conserves* the heat of the underlying part of the globe, so that the rate of waste or secular cooling is not so high under sedimentary areas as under

[1] Prof. Joseph Prestwich observes (Proc. Royal Soc. 1866): "With respect to the possibility of change in the thermometric gradient at great depths, it is known that the conductivity of wrought iron diminishes as the temperature increases." From this he infers that the thermometric gradient may increase at great depths.

the uncovered areas, and especially the areas subjected to denudation, for denudation will act, of course, in an opposite manner and depress the isogeotherms, but not at the same rate, as the mean rate of denudation—being over a larger area—is less, as I have shown, than the mean rate of deposition, which is over a smaller area.

And here I would point out that whether the covering material be aqueous sediment or volcanic ashes or lava sheets, the principle of conservation of the internal heat of the globe is the same. I think, however, that the non-conductive property of ashes would be much greater than ordinary sediments, and combined with lava sheets—whether extrusive and laid down on the surface, or intrusive like the Great Whin Sill—they would bring actual additions of heat as well as matter to the conservative covering.

As the larger part of the earth's surface, so far as we are enabled to examine it, is on the continental areas covered with aqueously derived sediment, sometimes interstratified with volcanic and plutonic rocks, and the remainder is covered with or consists of igneous rocks, such as granite or basalt, or of old gneiss or metamorphic rocks made out of any of these rocks, we at once see that we must not limit our view of the earth to that of an inert body cooling from its outer envelope into space, but of such a body subject to secular cooling and to the reactions brought about by meteorological influences and to the irregularities of cooling and consequent expansion and contraction which have taken place from the beginning of geological history, and is evidenced by all the surface phenomena with which we are acquainted.

The Shell of Compression.

We will now go back to the point from which we started and, looking at the globe as an inert mass cooling symmetrically into space, try and quantitatively examine whether the shell of compression, forced by its own gravitation to adapt itself to the shrinking globe, is sufficient to account for the corrugation of the earth's surface, together with its volcanic phenomena, as was hitherto held by most geologists, and still is by some, though in a hesitating sort of way.[1] I prefer to treat this in a very simple manner, so as to bring it within the comprehension of the ordinary mechanical mind. We have seen that, according to Lord Kelvin's results—in which, I believe, most physicists concur—the practical cooling of the earth only reaches down now about 150 miles. Let us assume that this shell, 150 miles deep, has cooled an average of 1000° since the beginning of Cambrian times. It has, of course, on the hypothesis cooled much more at the zone of greatest contraction, but almost nothing at 150 miles deep. It seems to me altogether too ample an allowance for loss of heat by secular cooling in the selected time, but, whether it be too little or too much, it will give us the sense of proportion we require to form our

[1] See Le Conte—"Theories of the Origin of Mountain Ranges," Nature, Oct. 5th, 1893, and my examination thereof, entitled "Genesis of Mountain Ranges," Natural Science, Nov. 1893.

conception. According to a series of experiments made by myself and recorded in the "Origin of Mountain Ranges," the mean expansion of the various rocks of which the earth is composed is about 2·75 feet per mile for every 100° F. This result is well within the mark on comparison with other investigations and, as I use the coefficient throughout, if it be not absolutely correct it will be true for purposes of comparison.

The linear contraction of 150 miles of rock cooled 1000°, using this coefficient, would be 4125 feet, but, as the contraction of the shell 150 miles deep is voluminal, the contraction in thickness of the shell would be three times this, or 12,375 feet, or=2·344 miles. This would be the radial contraction of the globe from the commencement of the Cambrian, which would give us a circumferential contraction of 77,715 feet, or 14·72 miles. This radial contraction is in excess of what Mr. Osmond Fisher, a well-known mathematician and geologist, has estimated, taking the temperature of solidification at 4000° F., for the whole geological history of the globe.[1] I wish to be liberal, and it will serve my purpose.

We thus have a linear lateral movement or circumferential contraction in the shell of compression of, say, 15 miles wherewith to build up all our mountain chains and produce the rugosities of the earth's surface that have been created since the beginning of the Cambrian. These rugosities do not all exist now, as they have been planed down again and again by denudation while otherwise growing from age to age. The highest mountains of the earth are of the latest formation, viz. of Tertiary age, and it is conceded by most geologists that they are so, not because they were initially higher, but because denudation has not been at work on them so long as on the mountains of preceding periods. According to the greatest estimate of the depth of the level-of-no-strain it is only at the present time 5 miles below the surface, so that the mountain-making capacity of the shell of compression ceases altogether at that depth.

This, of course, is utterly inconsistent with the production of the phenomena of mountain chains, where we find stratified rocks forced up to the surface from much greater depths, and the granitic and gneissic axes, which form a distinguishing feature of all great ranges, give evidence of deep-seated forces acting far below the stratified crust.

Again, the volcanic phenomena cannot be explained by the lateral pressure of the contracting crust, as these again, it is admitted, have a deep-seated origin.

When, therefore, we examine this theory of mountain ranges originating from compression of the crust induced by the shrinkage of the globe we prove it quite inadequate to explain the phenomena.

[1] Mr. O. Fisher, taking the temperature of solidification at 4000° F. and the time at 33 million years, arrives at a total radial contraction of 2 miles. If the temperature of solidification were 7000° F. and the time 98 million years, the total radial contraction would be 6 miles. These calculations cover the whole of the time since first solidification, whereas the time I am dealing with is from the beginning of the Cambrian, probably not half the time that has elapsed since the first solidification of the globe.—See Fisher, Phil. Mag. 1888, pp. 7–20.

On the contrary, we find that the cooling shell of the globe for a depth of 145 miles below the level-of-no-strain is actually contracting, but in degrees, varying with vertical position; that the stresses are tensile, rupture being avoided by the gravitation of the overlying masses closing up otherwise possible cavities by compressive extension.

But if we look at the globe as a mass which is not losing heat equably but is contracting at one locality and expanding at another, though still on the whole undergoing secular refrigeration, it will not help what is known as the "contraction theory" of mountain building. The effects calculated in this paper as possibly due to the condensation of the shell of compression will be less, more irregular, and difficult to trace out. Indeed, so much so that the disturbing causes will have far greater effect than general secular contraction, and while adding to the lateral movement in one place may efface it in another.

Effects of unsymmetrical cooling on the outer envelope of the Earth.

Having now, as far as my limited powers of exposition allow, stated for your consideration my views on the effects of symmetrical cooling on a homogeneous heated globe, comparable in size with our earth, and partly traced some of its possible geological effects on the outer envelope, it is time to consider quantitatively some of the disturbing agencies which are constantly at work to modify the sum of the results.

First then, and most important as initiating other movements, is that of sedimentation. Those who are not familiar with geology will pardon me for stating that the gross thickness of all the sedimentary formations of the earth is estimated by competent authorities at as much as 100,000 feet,[1] but it must be understood that these never occur together in one geological column. The statement is sufficient to invest sediments with considerable interest to physicists as modifying in several important ways reasoning based on hypothetical conditions, though they are hypotheses that are absolutely necessary to be made before the actual problem can be efficiently attacked.

The greatest actual thickness of sediment piled up in one spot in almost unconformable succession is found in great mountain ranges such as the Appalachians and the Alps, and has been estimated by competent geologists at from 8 to 10 miles.[2]

But below these is a mass of Archean rocks of unknown thickness which is certainly not the original unmodified crust of the globe.

The area of the sedimentaries is co-extensive with the continents

[1] Sir A. Geikie, Pres. Address British Association, 1892.

[2] Judd, speaking of the Alps (Volcanoes, p. 295), says: "The united thickness of all sediments accumulated along this great line of subsidence between the Permian and Nummulithic periods probably exceeds 50,000 feet or 10 miles." Various American authorities estimate that the thickness of the Palæozoic system of the Appalachians reaches 40,000 feet; the Palæozoics and Mesozoics in the Wasatch 50,000; the Cretaceous in the coast ranges of California 20,000 feet, and in Shasta county 30,000 feet; the Palæozoic and Mesozoic of the Uinta 30,000.—See Le Conte—Theories of the Origin of Mountain Ranges, Journal of Geology, 1893, p. 544.

and islands, and they occupy in addition unknown regions under the seas and oceans. The Cretaceous and Tertiary rocks cover, or have covered, at least half the continental area of North America, extending through 60 degrees of latitude and being a thousand miles wide in the United States.

It is in this area the Rockies and related mountain ranges have been thrown up, involving Cretaceous and Tertiary rocks in their folds.

For the purpose of illustrating the effects of sedimentation on the earth's outer envelope as compared with that due to secular contraction we will assume an area 2000 miles long, 1000 miles wide, and 10 miles thick along a central longitudinal trough, but gradually thinning in lenticular but irregular fashion towards the edges. Let us further assume that 4 miles represents the average thickness of this group of sedimentary strata. This would augment the temperature of the bed-rock on which the sediment rests by 400° at the rate of 1° F. per 50 feet. This accession of heat would represent a linear expansion in one direction of 22,000 feet and in the other of 11,000. So that we have for one group of rocks, over what is only a local area of the globe or $\frac{1}{2^1 6}$ of the total land surface, a linear expansion of 4 miles in one direction and over 2 miles in the other to contrast with the 15 miles surface contraction of the whole globe, including land areas and ocean, since the beginning of the Cambrian.

The shortening of this area, if the shrinkage of the globe were equally divided over its surface, would only amount to $\frac{5}{8}$ of a mile in one direction and $1\frac{1}{4}$ miles in the other, through all the time that has elapsed since the commencement of the Cambrian. But for a true comparison with our sedimentary example, taking into account geological time, we may divide this by not less than 5, reducing the shortening of the area on the hypothesis of contraction to respectively $\frac{1}{8}$ and $\frac{1}{4}$ of a mile. But when we come to contrast with this the effects of cubical or voluminal expansion instead of linear, it will be seen that the thermal expansion of the rocks due to sedimentation is still more in excess of any possible accumulation of volume due to the shell of compression as possibly affected by such contraction.

The increase of temperature is not, as I have before explained, limited to the sediments; it affects in turn the whole cooling shell below and raises its temperature in a corresponding degree. I think it will be quite fair to take this as equal to raising the temperature of the outer envelope 400° to a depth of 20 miles. These are only intended to be illustrative figures, applying to a small section of the earth in space and a small section in time. If we cube up our hypothetical block when expanded, and compare it with its volume before expansion, we shall find that we have in round figures 241,000 cubic miles for mountain building and other orographic changes, as against 26,000 cubic miles produced by the approach of the shell of compression over a similar area to a similar extent; while in the second case of $\frac{1}{8}$ and $\frac{1}{4}$ of a mile the disparity is so enormously increased as to show its absolute incapacity to explain any orographic features whatever.

Effects of Local Shrinkage through Denudation.

If instead of 4 miles mean thickness of sediment being laid down on the given area of 2000 by 1000 miles, we assume that the same mass has been removed from an equal area by denudation, and, supposing the crust to be free to shrink and could sustain its own weight, the area would be surrounded by trenches respectively 2 miles and 1 mile wide at the new surface. It necessarily follows that, even if the shell of compression were to follow the shrinking area up at the same rate, there would still remain cavities or trenches below the surface. Secular contraction it is evident could not be operative in producing compression in such a case. Local shrinkage of the crust such as we have assumed does not, however, act in this way. The weight of the crust itself squeezes up all vacuities, and the area adjusts itself to its decrease of volume by normal-faulting and keying up in a wedge-like manner and by compressive extension.

We have seen that the shell of contraction due to secular cooling is a real and important factor in the earth's economy. This general shrinkage decreases in rate in areas of sedimentation and increases in areas of denudation. It may perhaps be said that if these areas adjoin one will destroy the effect of the other. Such, however, is not the case, as I have shown that the expansion is internal shading off to zero at the boundaries. In the theory of the origin of the earth's folds by secular contraction it is assumed that the folded area is compressed from the outside; on such a supposition it is a perfectly legitimate inference that the shrinking area would be followed up by the closing in of the shell of compression, and one effect would destroy the other.

I further maintain that it is a physical impossibility for a layer of the earth 5 miles deep, really but a film upon the surface of the globe, not only to shear upon itself but to convey thrust through 1000 miles of strata in an effective manner. Even if we were to assume that this shell could shear upon itself and accumulate thrust in one place, the compression would be confined to a short distance from the boundary of the area and be inoperative in the centre where mountain ranges are usually thrown up.

The compression produced by expansion is *internal* and proportional to the depth of the sediment.

This is an important rule to bear in mind, for it largely determines the axial position of the range. The mere upheaval of some of the strata in anticlinal form will determine where the lateral pressure from the surrounding sheet of strata shall be used up. When one fold is compressed another follows up against it, and so the work of mountain building proceeds. From the comparatively great depth at which the internal expansion acts, underlying rocks are forced up into gneissic and granitic cores, and these expanding laterally by gravitation force back the folded strata and compress them still more, forming in many cases what is known as fan-structure. The very movement of these heated masses of rock forced up from below

brings further heated masses to increase the temperature of the sediment and crust below it. I have shown experimentally that every variation of heat which takes place goes for mountain building, an increase producing a further compression of the folds and a decrease compressive extension or normal faulting. But I feel that time is short, and I must conclude without oppressing you with further details.

Résumé.

We have thus seen that changes caused by a fluctuating and irregular distribution of temperature in the crust through unsymmetrical cooling is a much more potent factor in producing the rugosities of the earth's surface than symmetrical secular contraction. There are other phenomena that it is necessary to take into consideration in any complete theory of mountain formation, but I shall merely note them here. The fact of the existence of piles of sediment 10 miles deep, or twice the depth of the deepest parts of the existing oceans, shows that the crust has sunk in such a locus to an abnormal extent. That this is due partly to the weight of the sediments is admitted by most geologists.

With a shell of contraction of the thickness I have shown, it is easy to see that any irregular distribution of weight must affect the crust by bending it, and this means also a disturbance of heated matter below. All these disturbances lead to heating and cooling of local areas, and this I have shown is the most potent cause of orographic changes. These lateral expansions take the form of ridging up; matter is actually moved towards the axis of expansion so that the ridgings up become permanent features until removed by denudation. The result of all the investigations of underground temperature show that the rate of augmentation downwards is very variable in different localities. We also know from the existence of volcanic tracts that the heated matter of the globe is there nearer the surface, and I have little doubt that the increase of heat downwards in certain areas is much greater than 1° F. per 50 feet.[1] No doubt the cooling of the earth takes place *on the whole* as inferred by Lord Kelvin; but sedimentation as one of the conditioning agents has not received the attention its importance demands. The earth, as I have shown, is covered with sedimentary matter in thickness to be measured by miles. Sediments many miles thick have been laid down, upheaved, crumpled and folded, denuded and again covered up by other sediments, the ruins of the old, often with volcanic additions from below.

The crust of the earth is in a constant state of change, none the less effective because slow as measured by our conceptions of time. The unloading of one portion of the earth's crust and the loading of another, the checking of the outflow of heat by sediments in one area and its acceleration in another by denudation, the movement of

[1] Prof. Joseph Prestwich, in an exhaustive discussion of underground temperature observations, suggests that 45 feet per degree is nearer to the true normal.—Proc. Royal Soc. 1886.

fused or semi-fused matter in the crust, partly consequent on the repeated heating and cooling of the outer envelope and a lateral thrust produced at each rise of temperature, with faulting and wedging up to compensate for contraction at each fall of temperature, are in my view the potent causes of the external forms of the earth as they have appeared from age to age.

IV.—On a Second British Species of the Jurassic Fish *Eurycormus*.

By A. Smith Woodward, F.L.S., F.G.S.

THE occurrence of a fish generically identical with *Eurycormus* of the Bavarian Lithographic stone, in the Upper Jurassic of England, has already been indicated by the discovery of a well-preserved head with some anterior vertebræ in the Kimmeridge Clay of Ely.[1] Only one specimen, however, has hitherto been recognized; and during a recent search for further evidence of the fish among British fossils the present writer has thus been gratified to find two more examples in the collection of the British Museum. The one specimen was described many years ago by Sir Philip Egerton, under the name of *Macropoma Egertoni*, and is said to have been obtained from the "Gault, Speeton"; the other specimen is an obliquely-crushed head, with part of the squamation, found by Mr. Alfred N. Leeds in the Oxford Clay of Peterborough. So far as they can be compared, these two fossils agree in every particular, even in detailed measurements; hence they must be referred, for the present at least, to one and the same species.

Eurycormus Egertoni (Egerton).

1844. *Macropoma Egertoni*, L. Agassiz, Poiss. Foss. vol. ii. pt. ii. p. 174 (undefined).
1858. ,, ,, P. M. G. Egerton, Figs. and Descript. Brit. Organic Remains (Mem. Geol. Surv.), dec. ix. No. 10. pl. x.
1866. *Eurypoma Egertoni*, T. H. Huxley, *ibid.* dec. xii. p. 32.

Description of Type Specimen.—The original description of the type specimen published by Egerton is so unsatisfactory, and gives so false an impression of some of the most important features of the fossil, that it seems advisable to attempt a new account. The head and anterior abdominal region are exposed in side view, as shown in Mr. Dinkel's drawing (*loc. cit.* 1858, pl. x. fig. 1); but the specimen is so much distorted by crushing, and fractured in front, that the apparent profile of the skull is entirely deceptive. The head is compressed by accident from side to side; the small left supra-temporal plate and the larger post-temporal are displaced upwards and forwards (the hinder margin of the post-temporal being described as "occiput" by Egerton); and the greater part of the rostral region is broken away, giving a false idea of steepness in profile in advance of the orbit. The supra-temporals form a single pair of triangular plates, and the occipital margin of the skull is evidently straight.

[1] Smith Woodward, "On a Head of *Eurycormus* from the Kimmeridge Clay of Ely," Geol. Mag. [3] Vol. VII. (1890), pp. 289–291, Pl. x.

The cranial roof-bones are very coarsely rugose and partly tuberculated; the boundaries of the squamosals are distinct. The sclerotic capsule is ossified; there is evidence of large post-orbitals; and the long, narrow supra-maxillary bone is conspicuous. The maxilla is smooth, more than twice as deep behind as in front, exhibiting the usual re-entering angle in the hinder border, and beset on the oral margin with a single, regular, close series of small slender teeth. The dentary-bone, so far as preserved, is also smooth, and on the right side three or four of the shallow sockets for the large teeth of the anterior extremity are distinctly exhibited. The rami of the mandible are much pressed together, and between them there appears the large azygous jugular plate, which extends backwards at least as far as the hinder extremity of the dentary. Behind the jugular plate there follow the branchiostegal rays; and the large bones of the opercular fold are as obscure as indicated in Dinkel's figure. Remains of vertebral centra are distinctly shown at the posterior end of the fossil, either as complete rings or as half-rings; and there are long robust ribs. The fragmentary base of the dorsal fin is evidently crushed forwards by the pressure which displaced the left supra-temporal bone; and the large pectoral fin is too imperfect for detailed description. It can be seen, however, that the pectoral fin-rays are dichotomously branched and articulated distally. The ornamented scales, as displayed especially in the dorsal region, are well described and figured by Egerton.

Description of New Specimen.—The new fossil from the Oxford Clay of Peterborough (Brit. Mus. No. P. 6912) exhibits less of the trunk than the type and is obliquely crushed from above downwards. The cranial roof is much fractured, but agrees in every feature that is comparable with the foregoing specimen, the rugosity being especially marked. As preserved, the cheek-plates, opercular bones and post-temporals are darker in colour than those of the cranial roof, comparatively smooth and with sparsely arranged fine tubercles. On the left side the characteristic hinder extremity of the maxilla is preserved, and on both sides there are traces of the long supra-maxillary bone. On both sides also there are remains of large ant-orbital and post-orbital plates. The mandible on the left side shows the front border of the coronoid elevation and part of the series of large teeth in the dentary bone; while on both sides there are traces of minute splenial teeth opposed to somewhat larger teeth on an inner element of the upper jaw. The left pre-operculum is exhibited, showing a considerable expansion in its lower half, not much bent forward, and externally ornamented with rugæ and tuberculations. The sub-operculum appears to be about twice as broad as deep, and both this and the operculum are marked with very fine tuberculations. There are no certain traces of vertebral centra or ribs; but it seems probable that an annular elevation beneath the scales at one point on the ventral aspect is a displaced ring-vertebra, while some rods seen in cross-section at the back of the fossil may possibly be ribs. Only a small portion of the clavicle remains on each side, smooth on its external face, longitudinally

ribbed at its anterior margin; and nothing more than part of the base of the pectoral fin can be observed on the right side. The scales are beautifully preserved and resemble those of the type specimen.

Specific Determination.—The length of the head in each of the fishes thus imperfectly indicated would be about 0·13m., and the species represented is therefore much larger than the typical *E. speciosus* of the Bavarian Lithographic stone.[1] According to the original description, the last-named fish is also distinguished by the smoothness of all the external bones. In size and characters the head of *E. Egertoni* agrees much more closely with that of *E. grandis* from the English Kimmeridge Clay. From this, however, it also differs in the remarkable rugosity of the cranial roof-bones, and apparently in the greater breadth of the squamosal elements as compared with the parietals. *E. Egertoni* is thus the third known species of the genus.

Formation and Locality.—Although the type specimen is said to have been obtained from the "Gault" of Speeton, Yorkshire (presumably the Speeton Clay), the horizon cannot be determined with certainty, even if the record of the locality be correct; for the fish is contained in a mass of indurated clay that has been much waterworn, evidently by rolling on the beach, and the matrix exhibits no invertebrate fossils by which its age can be recognized. The fact that it agrees exactly with the new specimen from the Oxford Clay of Peterborough is almost certain proof that the original *E. Egertoni* is not a Cretaceous, but an Upper Jurassic fossil; and the discovery of further specimens in the Yorkshire cliffs will be awaited with interest for the settlement of the stratigraphical question.

Eurycormus, sp.

Eurycormus will probably soon be recognized as having a still wider range in British Upper Jurassic formations than is now proved; and it is worthy of note that certain vertebral rings and hypocentra, not uncommonly met with in the Kimmeridge Clay of Weymouth, are more closely paralleled by those of this genus than by those of any other known fish. One such specimen (Brit. Mus., No. P. 6176) was figured by Mr. Damon,[2] and there are several others in the British Museum (Nos. 41181, 41231, 45926). There is one detached horse-shoe-shaped hypocentrum (No. 41231*a*) bearing a pair of pedicles (each perforated transversely by a foramen) for the support of ribs; and some of the complete rings exhibit a feeble oblique line on each side, as if formed by the coalescence of horse-shoe-shaped hypocentra and pleurocentra. These rings also bear a small pedicle or raised facette on the lower part of each side.

[1] A. Wagner, "Monographie der fossilen Fische aus den Lithographischen Schiefern Bayerns," Abh. k. bay. Akad. Wiss., math.-phys. Cl. vol. ix. (1863), p. 707, pl. iv. A smaller fish, probably to be regarded as the young of *E. speciosus*, is also named *E. dubius* by B. Vetter, Mitth. k. mineral.-geol. Mus. Dresden, pt. iv. (1881), p. 113, pl. ii. fig. 7.

[2] R. Damon, "Geology of Weymouth," ed. 2, suppl. pl. xii. fig. 9 (1880).

V.—On Chlorite as a Source of Biotite: a Reply.

By Charles Callaway, D.Sc., M.A., F.G.S.

I AM much obliged to General McMahon for criticising my recent paper in the Geological Magazine (Dec. 1893, p. 535) on the "Conversion of Chlorite into Biotite," because I have been puzzled to know the exact nature of the opposition to my views on rock-metamorphism at Malvern, and I have been haunted by the fear that perhaps after all I might have overlooked something fundamental. Happily my curiosity is now gratified, and my fear is removed, so far as one important branch of the enquiry is concerned. As my critic is so good a chemist, I may conclude that the worst has been said that can be said from the chemical point of view.

The title of General McMahon's paper, "The Rape of the Chlorites," is a little startling. One is reminded of the author of *Nana*. I must confess to a little grief that any chemical or mineral pets of mine should be charged with impropriety and lawlessness, and I shall try to show that such aspersions are unfounded.

In his second paragraph my critic concedes a great part of my theory on the origin of biotite. Referring to the authorities quoted by me in support of this theory, he states that two of them deal with "cases of contact-metamorphism." He apparently agrees with these writers, for he goes on to say: "In cases of contact action, one can readily understand how aqueous acid vapours, or liquids, emanating from the molten igneous rock under high pressure, penetrated the adjoining rocks, and carried with them in solution some of the constituents of the igneous magma." It would therefore appear that General McMahon admits the conversion of chlorite to biotite by contact action; and, if so, I want to know why he opposes my theory.

For the last four or five years I have been insisting that, in the Malvern crystallines, biotite has been produced out of chlorite by "contact action." In 1889 I wrote:[1] "The most obvious fact observable in the field is the production of the biotite. Granite-veins, if of large size, that is, six inches or more in diameter, are often surrounded by a sheath of kersantite two or more inches in thickness. Indeed, so uniform is the effect of the contact that the vicinity of veins may usually be safely inferred from the appearance of biotite in the diorite." I do not think I could have spoken more distinctly than this.

Now if, as I understand my critic to concede, the hydrous mineral chlorite may, even in the presence of "aqueous vapours and liquids," be transformed into the anhydrous mineral biotite in cases of contact action, why should it be thought necessary to confine the contact effects to the exact point where the intrusive vein touches the enclosing rock? Contact effects extend to considerable distances from the intruding mass in Cumberland, in county Dublin, and elsewhere, and why should they not extend to a few yards' distance

[1] Quart. Journ. Geol. Soc. Aug. 1889, p. 487.

at Malvern? In the shear-zones, where most of the biotite is formed, there is usually a plexus of granite-veins, and the diorite is so minutely sheared as to become in a high degree pervious to fluid injections. Where, then, is General McMahon's difficulty? So far as I can see, he has created it for himself. Let me elucidate this point.

After describing the nature of contact action, my critic goes on to assert that "in the case supposed by Dr. Callaway the conditions are altogether different." Are they? Whence has General McMahon derived his information on the point, if not from my papers? But I have never described the conditions as "altogether different." On the contrary, I think it probable that there is not a scrap of biotite in the crystallines of the Malverns which has been produced except by "contact action." I have, of course, dwelt upon the important effects of dynamic agencies; but I have insisted with equal emphasis upon the changes brought about by the intrusive granite-veins.

I may observe in passing that General McMahon attributes to me an assertion that in rock metamorphism the combined water of the chlorite has been driven off by heat. This is not quite accurate. In my last paper, I quoted the fact that in the laboratory chlorite loses its water at a red heat as a "proof that the affinity between the water and the other constituents of the mineral is not very strong"; which is quite another matter. I have not presumed to offer a complete account of the operation of the forces that were working deep down in the crust in Archæan times. Heat, chemical energy, and pressure no doubt co-operated, and who knows what other causes may have assisted in the schist-making?

General McMahon's amusing metaphor of the "Rape of the Chlorites" is suggested by a theory of the process of metamorphism which has no basis in my writings. He supposes that his maidens, the chlorites, are in one part of the rock regarded as highly heated; and his Roman soldiers, the potash and iron,[1] in another part, considered to be much cooler. This could by no possibility have been the case. The soldiers and the maidens were living in the same house, only waiting till the affinity between them should grow strong enough to draw them into the bonds of lawful wedlock. My critic's misapprehension seems to have arisen from a confusion as to the order of events in the schist-making process.

I have frequently described sections which show a passage between a diorite and a biotite-gneiss. Where the diorite is slightly crushed, there is a good deal of liberated iron-oxide. Further along the section, where biotite begins to appear, the visible iron decreases in quantity, having been partially used up, as I have elsewhere shown, in the genesis of the biotite. General McMahon says that this iron (together with potash) has got to rush from one locality to another, like Roman soldiers sacking a town. But this is pure imagination. Surely it is quite obvious that the succession of events which we now witness in geographical series was once a

[1] I have not taken any account of the alumina, since the percentages of that oxide in the chlorite and the biotite are roughly equal.

succession in time on the same spot. A mass of diorite begins to suffer from a crushing process, aqueous solutions, in which both carbonic acid and potash occur,[1] soak into it, and chemical changes take place. The pressure increases, shearing supervenes, the temperature rises, and certain other chemical reactions result. There is no rushing about from one place to another. All the materials for the genesis of the biotite are already present, before the higher temperatures set in.

To be more precise I will briefly state some of the actual changes, as observed in the field and the microscope, and confirmed by chemical analysis. Crystals of hornblende and felspar are lying side by side. Some of the latter contain potash, perhaps as the result of contact action. Crushing sets in, and foreign solutions enter the rock. The crystals of felspar are broken to fragments, and some of it is decomposed, alkalies and lime being liberated. The hornblende also is crushed and decomposed, chlorite, iron-oxide, and calcite being produced. The materials for the production of the biotite are present, but increased heat and pressure are necessary to excite the required chemical affinities. In an advanced stage of the metamorphism, the water of the chlorite, and some of its magnesia, are driven out, and their place is taken by iron-oxide and potash. That, at high temperatures, iron has a strong affinity for silica, is proved by the well-known fact that, in the consolidation of igneous magmas, iron-silicates come very early in the order of crystallization. According to Lagorio they come next to oxides and before any other silicates.

I do not think that General McMahon will attach much importance to his objection that, if the temperature of the rock "rose to the point of fusion," the chlorite must have been melted down and merged in the general magma, and, therefore, could not have given rise to biotite. It is sufficient to remind him that the fusing-point of chlorite is much higher than that of felspar, and that, in granite, quartz usually consolidates after felspar. It would, therefore, be exceedingly rash to assume that a temperature which would fuse quartz and felspar,[2] which form the bulk of the rock, would also melt the chlorite. My assertion that, in scores of sections, chlorite is seen to be replaced by biotite as we approach the granite-diorite shear-zones, will hardly be invalidated by such a speculative objection as this.

My critic denies that the conversion of chlorite to biotite is "an observed fact." But, surely, if Lossen, Michel Lévy, Salomon, and Rüdermann, not to mention my humble self, have observed the change, it may fairly be regarded as proved. Besides, I thought that General McMahon himself admitted that the thing might be true as the result of "contact action." I confess I do not quite understand his position.

[1] Not necessarily contemporaneously as potassium carbonate.

[2] Professor Redway, of Mount Vernon, New York, commenting on my Malvern papers ("Science," February 9th, 1894), describes the fusion in the shear-zones as "sufficiently complete to produce plasticity."

This question of rock-metamorphism is deserving of full consideration. If I have not made myself clear to my critic, or to any other of the readers of this MAGAZINE, I shall be glad to give further explanation; and, if I am wrong, I will engage to surrender to General McMahon and his legion of Roman soldiers.

VI.—On Variolite and other Tachylytes at Dunmore Head, Co. Down.

By GRENVILLE A. J. COLE, M.R.I.A., F.G.S.
Professor of Geology in the Royal College of Science for Ireland.

WHILE examining the dykes along the coast of Mourne in the company of Mr. R. Welch, I came across an additional example of intrusive variolite, the occurrence of which seems worth recording. The mass is, in this case, a thin one, about 20 centimetres in width, narrowing and dying out as it is traced northwards; it runs in a somewhat sinuous course along the strike of the uptilted strata. Every other dyke that I have seen upon this richly favoured coast cuts across the Ordovician beds, and the present instance to the contrary may be only a lateral sheet-like offshoot from one of the familiar basalts.

This variolite occurs on Dunmore Head, just south-east of the summit of Dunmore Hill, and a little below the level of high-water. The handsome and massive variolite of Annalong[1] lies some $2\frac{1}{2}$ miles to the south.

The thin intrusive sheet is minutely spherulitic upon its selvages, but passes rapidly into a typical variolite, with spherulites 3 mm. in diameter. These bodies weather out in pale tints against the dark grey-green groundmass. The specific gravity of a large typical specimen is 2·86, being nearly as high as that of the variolites of the Western Alps (2·90), while the rock of Annalong yields only 2·72.

In the example from Dunmore Head we have a variolite obviously connected with ordinary tachylyte, and it seems unnecessary to again urge the relationship between the two rocks, variolite being merely an altered form of the more coarsely spherulitic types of basic glass. Professor Zirkel,[2] in preferring to regard the rock of Annalong as a "sphaerolithführendes Diabasglas" rather than as a true variolite, practically admits the impossibility of maintaining variolite as a distinct rock-species. The name is a convenient term for specialists, like the corresponding word "pyromeride," in the classification of highly silicated rocks; and it has by far too great an antiquarian interest to be thrust aside into oblivion.

At Annalong the dyke is bounded by a film of tachylytic glass 5 mm. in thickness; but the main mass is an indubitable variolite. The rock of Dunmore Head has an altered groundmass, with traces of glass on the extreme selvage; but the spherulites are far more fresh and interesting than in the variolites that I have hitherto examined. Under the microscope they are seen to be composed,

[1] Proc. Roy. Dublin Soc. vol. vii. (1892), p. 511.
[2] "Lehrbuch der Petrographie," 2te Auflage (1894), 2te Band, p. 704.

not of feathery bunches of felspathic rods, but of "cryptocrystalline" material with a delicate radial structure, thus resembling more closely than usual the spherulites of rhyolitic rocks. Their colour in reflected light is greyish-white, and in transmitted light grey-brown and yellow-brown; a certain amount of magnetite has formed in dusty aggregations round about them, or along the lines of junction of their component groups of fibres. As has been observed in the spherulites in the tachylyte of Ardtun, the yellow-brown material is distinctly pleochroic.

Cracks traverse the spherulites freely, passing also across the groundmass; but I have found no trace of the "pseudo-crystals" that are so prominent a feature in Continental variolites. The cracks have originated from subsequent earth-movements, probably at the time of the intrusion of the great granite masses of the Mourne Mountains, which cut off so many of these coast-dykes abruptly.[1]

Some of the spherulites, however, have been broken up by the fluid glass, which has entered in between the detached fragments and has formed a microscopic network of intrusive veins. In several cases an early phase of the rock is revealed to us by the occurrence of crystals of plagioclase as nuclei to the spherulites. These crystals have been greatly corroded by the magma round them, and many of them must have been entirely absorbed. The glass intruded into them is crowded with globulites; but its present condition does not enable us to judge of the character of the original unaltered magma.

The altered glass forming the groundmass of the variolite is, as usual, greenish; in large part it is chloritized, but epidote has only locally developed. A minute spherulitic and cumulitic structure was originally present in it, and may be very generally traced. Perlitic structure may have also occurred, but it is only obscurely discernible in one of the six sections that I have studied.

This rock gives no evidence as to whether it is the spherulitic representative of a basaltic andesite or of an olivine basalt. I have no hesitation in classing it as a variolite, and it is, I believe, the fifth known representative of this rock in the British Isles, the others being in Anglesey, the Lleyn, Co. Wicklow (near Roundwood), and at Annalong. It may perhaps be one of the "variolites" noted in 1835 by Major Patrickson, who names the rock once or twice in the region to the north of Annalong.

A very compact black rock, with minute spherical amygdaloidal vesicles, occurs as a thin dyke between Dunmore Head and Green Harbour. It has the high specific gravity of 2·93. In section it approaches in beauty the rhyolitic pitchstones of Arran, while at the same time revealing its more basic character. Feathery bundles and sheaves of felspathic materials are associated in the pale-green altered glass with long curving rods and plumes, presumably of pyroxene and magnetite. A few porphyritic crystals of plagioclase are again the only constituents sufficiently developed to be identified with absolute confidence.

[1] "Dykes appearing on the shore which skirts the Mourne Mountains." Journ. Geol. Soc. Dublin, vol. i. p. 182.

Another section, from the selvage of a large basaltic dyke in the same area, is a superb example of palagonite, the altered tachylyte having all the clear brown translucent character that is familiar in Icelandic and Sicilian types. Spherulitic aggregates of a more dusky brown colour began to form at one stage, but were in part redissolved in the fluidal groundmass. Abundant oval yellow-brown bodies, dark upon the exterior, occur in the palagonite, and are probably the minute spherulites of the last phase of consolidation, deformed by the flow at the edges of the dyke.

It is only by the brilliant play of colours when the section is viewed between crossed nicols that one discovers that this beautiful and apparently unaltered tachylyte has passed thoroughly into the palagonitic and micro-crystalline condition.

Enough has now been noted to confirm what was remarked two years ago as to the frequency and variety of vitreous andesites and basalts on this interesting coast of Mourne. I still feel that in all probability this assemblage of dykes is of early Eocene age.[1]

VII.—An Ancient Glacial Shore.

By Joseph Lomas, A.R.C.S., F.G.S.

FOR some time I have had under observation the sections of drift exposed in the cuttings of the Seacombe branch of the Wirral Railway, and hope to describe them on some future occasion when the sections are nearer completion.

However, Mr. Mellard Reade has forestalled me[2] in the description of one part of the sections, and professes to have found in them evidence of "an ancient glacial shore." What does he produce in favour of the statement?

In the first place "the bed is composed of pretty clean sand with some small gravel, and is crowded with shell fragments in all stages of decay." Secondly, it is of large extent and has a gentle slope. Thirdly, rolled clay balls are found mixed with the sand.

The bed in its composition does not differ from the ordinary sands and gravels found so commonly in the glacial deposits of Lancashire and Cheshire.

Some of these contain rolled clay balls (though I have never met with a section which displays them in such abundance). Some equal or exceed the present example in extent. They often contain shell fragments, which are sometimes planed and striated like ordinary boulders. It is noteworthy, too, that the shelly deposits only occur in places where we can prove by independent evidence (such as boulder transport and glacial striæ) that the ice to reach that place must have traversed a sea bottom.

The beds are usually lenticular in shape, slope at all angles and occur at all horizons. Two or three may often be seen above each other in one section. Shall we consider all these as "ancient glacial shores?"

[1] See Proc. Roy. Dublin Soc. vol. vii. (1892), p. 518.

[2] Geol. Mag. Feb. 1894.

Some years ago I described before the Glacialists' Association beds of gravel containing rolled clay balls, occurring at Offerton and Romiley in Cheshire. These beds form the cores of glacial mounds which can be exactly matched by mounds now being formed by glaciers in places the sea does not reach.

The most ardent supporters of the submergence hypothesis would, I think, hesitate before attributing such mounds to shore action, even though they contain clay balls.

Is the presence of clay balls in a deposit proof of shore conditions in a tidal sea? Running water may be essential to their formation, but surely there would be enough of that in the drainage system of a great glacier.

But we are told "if they were constantly immersed they would get dissolved." Why have they not been "dissolved" out in their present situation? The conditions could hardly have been more favourable, for the gravel lies between two impervious beds of boulder clay and forms a natural line of drainage.

The reason is best given in Mr. Reade's own words. The "balls are covered over their surface with small gravel, shells and sand." They are thus armour-plated, and Sir Henry de la Beche[1] pointed out the significance of this fact long ago. He shows that clay containing fine gravel would not be acted on by running water with a velocity of less than 12 inches per second.

It is then no question of getting "partially dried during recession of the water twice in 24 hours," and their presence in a bed affords no evidence of tidal action.

Glacialists will thank Mr. Reade for responding to their request to show them the shore lines of the supposed glacial sea, but to be convincing they must be supported by evidence different from that given in his paper.

Postscript.—Since the above was written an article has appeared in the *Glacialists' Magazine* (March, 1894) by Prof. N. H. Winchell, State Geologist, Minneapolis, Minn., on "Pebbles of Clay in Stratified Gravel and Sand."

He gives several instances of clay balls occurring in stratified drift in America, and in places where marine action was quite out of the question.

He concludes that clay balls "may be produced and embedded in gravel and sand which was the direct result of the wastage of the glacier, and that they are not unquestionable evidence of the former action of an oceanic shore line."

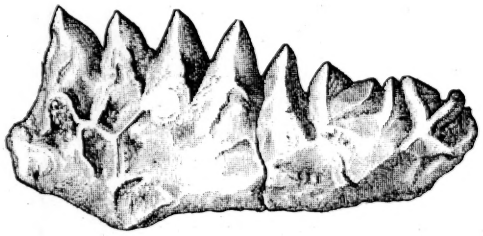
VIII.—On the Occurrence of Pigotite in the Caves near Porthcurnow, St. Levan, Cornwall.

By J. K. Creighton.

HAVING heard from my esteemed friend and colleague, Mr. J. W. Wetherell, that some years ago a brown crystalline substance had been found in a cave close to the sea, and situate

[1] Geological Manual, p. 51.

between Porthcurnow and Porthchapel, I was impelled to make a search for the mineral. Last July I succeeded in finding a cave near Porthcurnow, the small opening into which was partially hidden by masses of fallen rock; here I found after a rough examination traces of the mineral for which I was looking. In the course of the next few days I made a complete survey of the cave, which is about 75 feet long and 15 feet wide, and varies from 10 to 15 feet in height. At some distance from the opening is a large block, which permits only little light to reach the farther end of the cave; the greater part of the block is covered with a



crust, varying from 1 to 15 lines in thickness, of the mineral in question; the crust had apparently been a result of the continuous drip of water from that part of the roof which is immediately above. Other crusts found at the farther end of the cave were from 10 to 30 lines in thickness. As the nature of the mineral was not known to anyone in the neighbourhood, I sent a fragment to the Natural History Museum at South Kensington, where it was examined by Mr. Fletcher. He sent me the following information:—"The dark brown fragment is opaque in the mass, but transmits light of a rich red colour through its thinner edges; it has the lustre, fracture, brittleness, low specific gravity, and indeed the general aspect, of an ordinary resin; to one's surprise, however, a flake held in a gas-flame refuses to burn. Heated on a platinum lid to a red heat, a fragment slowly burns away, and, after final heating with the blowpipe, leaves a grey ash; heated in a glass-tube open at only one end a fragment gives off much water and empyreumatic products, and leaves a black mass behind. The powdered mineral is of a yellow colour. These characters and behaviour indicate that the substance you have sent me is identical with a mineral described before the Royal Society more than fifty years ago by Mr. James F. W. Johnston, M.A., F.R.S., and named by him Pigotite after the Rev. M. Pigot, the co-discoverer."

Mr. Fletcher at the same time sent me a copy of Mr. Johnston's paper, from which I find that the resin, "which had been found as an incrustation on the sides of certain caves occurring in the granitic cliffs on the east and west coast of Cornwall," was regarded as a chemical compound of alumina, a new organic acid (mudesous acid) and water. As regards its mode of origin Mr. Johnston says

"the organic constituent of this substance may be considered to be derived from the decay of the various plants which grow on the moist moorlands above, and which, being carried by the waters into the fissures of the granite beneath, combines with the alumina of the decomposed felspar; and when it reaches the air deposits itself on the roof and sides of the caverns in the form of layers, varying from a line to two or three inches in thickness."

The mode of occurrence in the cave near Porthcurnow agrees with the description given by Mr. Johnston: over the cave grow furze and bracken, and in the cave itself is a remarkable moss; fissures are visible in the roof at the farther end. The back of the cave gradually slopes towards the above-mentioned block.

The Pigotite is also found on the roof of the cave, but there has a different form, the surface being rippled and the ripple-marks being in some cases most distinct in character; in rare cases the form is serrated or dentated, and is suggestive of a mimicry of certain fossil remains, reminding one of the teeth of *Ceratodus* from the Rhætic beds (see Woodcut, p. 224). The frangibility of the mineral makes it difficult to obtain large specimens of any kind.

In the following month, from another cave only accessible at certain tides, I obtained a small amount of Pigotite with somewhat different characters; it was more transparent, and in some cases of a rich yellow colour, in others ruby red; this kind has the general form of an efflorescence.

It may be hoped that the rediscovery of this mineral may induce some chemist to make a more complete examination of so extraordinary a compound.

NOTICES OF MEMOIRS.

On the Structure of the French Alps. By Professor Marcel Bertrand. Translated from *Comptes Rendus*.

FOUR years' work in the Maurienne and Tarentaise at first led me to adopt the views of our Italian colleagues with regard to the "lustrous schists," and to consider them Palæozoic in age. This year, however, I have found conclusive sections which compel me to return to the old opinion of Lory, and to assign them to the Trias and even in part to the Lias. The proofs, which I shall give elsewhere, are closely connected with general conclusions on the structure of the region studied, and may even I believe extend to the whole chain. It is these which I wish to enumerate briefly here.

(1) The French Alps possess fan structure. The Carboniferous belt, stretching from Bourg St. Maurice to Briançon, is the centre of the fan. Along its edges on either hand there is a sort of narrow frontier zone in which the exact direction of overthrusting is uncertain; but, once this zone is passed, all the folds on the east

side incline towards Italy, and all those on the west towards France.[1]

The exceptions to this rule, such as the fans of Mont Blanc and St. Gothard, are infrequent and accidental. Side by side with classic sections of these massifs, which show fan structure, I believe that one could cite others, parallel, where it is wanting. Besides, such exceptions are only to be seen on the flanks of the "amygdaloidal massifs," to which I shall shortly refer.[2]

Towards the north-east on reaching the (Swiss-Italian) frontier the central zone swells out considerably, so as to embrace *the whole Monte Rosa massif*; in this broadened zone the folds are not inclined in any one determinate direction.

On the south the zone undergoes a more extraordinary change; instead of being occupied, as seems natural, by the most ancient rocks arranged in an anticlinal, it is made up of the most recent rocks, the nummulitic beds, and the flysch. *The central zone is marked, geologically speaking at least, not by an uplift but by a depression.* A preliminary note by MM. Kilian and Haug has told us that in this new region (nummulitic band of Ubaye) there seem to be peculiarities altogether unique in the Alps—inclined folds of pre-Eocene date. Possibly these peculiarities can be correlated with the great and sudden lowering of the central zone of the fan. However that may be, the Eocene band, from the point of view of the inclination of the folds, certainly plays the same part as the Carboniferous belt; all the western folds incline towards France, all the eastern ones towards Italy. Moreover, further on, the Mercantour massif, rising within the Eocene belt, marks a temporary return to a sharply anticlinal form. It seems to me, on comparing the folds of which the bearing is known in the intervening region, that the centre of the Alpine fan corresponds exactly with that of the Pyrenees.

(2) The plan of the folds in this part of the Alps shows *an "amygdaloidal or beaded structure"* (une structure amygdaloïde ou en chapelets), that is, the folds, while following the general trend of the chain, open out here and there round elliptical lenticles, themselves broken by new folds which have the same direction but are not prolonged beyond them. The system thus presents a series of nodes and ventral segments comparable with the foliation of an augen (amygdaloïde) gneiss, in which the folia bend round great kernels (noyaux) of quartz and felspar.

To give an idea of the importance of such kernels I will cite first

[1] It is by virtue of this fan structure that the relatively recent date of the "lustrous schists" can be demonstrated. Throughout almost the entire range of these schists order of superposition proves nothing, because the structure is uniclinal. Fortunately certain great patches are still left on the summit of the fan. Thus not only is evidence of superposition perfectly clear, but it is such as to allow no other interpretation.

[2] There would be, it is true, an important exception if the massifs of Annes, the Chablais, the Faulhorn, and the north fold of Glarus are, as is usually maintained, true anticlinal masses. This exception would disappear if they are overthrust masses. Thanks to the work of M. Schardt, attention is now definitely drawn to this problem, which I believe to be on the eve of solution.

the example of Mont Blanc, a great kernel rising in the middle of a Liassic syncline, which shuts in round it. In the region to which I have devoted special attention, the Vanoise, Mont Pourri, with the Aiguille du Midi, the massif of the little Mont Cenis, and probably that of the Grand Paradis, all owe their existence to a like phenomenon; on the other hand, the massif of the great Sassière, and that of the Sana, are due to the sudden broadening of synclinals; in place of the ancient rocks it is the most recent ones which are here developed, but the system of folds sweeps round these widenings in just the same fashion and opens out and shuts in round them.

I do not think that we have, at least on a large scale, anything strictly comparable to this *amygdaloidal structure*, unless it be the crystalline region of the Lake of the Woods in America. Mr. Lawson derives one of his arguments from this structure to prove that the kernels formed of granitoid gneiss are eruptive in origin. Without contesting Mr. Lawson's conclusions on this ground we may point out from the structure of our Alps that this argument, if it stood alone, would be valueless.

(3) A third conclusion is the generalization of that which M. Termier has already drawn from the study of Vanoise. *On the east of the Carboniferous band metamorphism increases from west to east.* I am driven, as was M. Termier in the Vanoise, to place confidently in the Permo-Triassic the ancient chloritic gneisses and mica schists of Mont Pourri, little Mont Cenis, and of the Val Grisanche, a continuation of the "*Casanna schists*" of Gerlach. Most of these arguments apply to the augen gneisses of the Grand Paradis, that is those which have been designated central gneiss; this would be a last term of the same metamorphism. It is to be noticed that in certain masses like the Dent Blanche and Monte Rosa we see, according to Gerlach, rising like an anticlinal from under this gneiss (or Casanna schist) still more ancient gneiss pierced by numerous granite veins, whilst such veins are absolutely wanting in the massifs previously cited; this is certainly a new argument. No region offers a more favourable field for continuing those micrographic studies so brilliantly inaugurated by M. Termier in the Vanoise, and it is to be hoped that we may be able to follow up in the formation and arrangement of the minerals all the stages to what it is at present convenient to term true gneiss.

This conclusion is analogous to that which M. Lossen has established for the ancient Hercynian Chain, where the Devonian in a similar central zone, situated, however, rather south of the centre, takes the form of mica schists and gneiss. It is right also to recall that M. Suess in 1869 made out a link between certain gneisses of the Carnic Alps and the Casanna schists in attributing a Permian age to them both.

I think it right to add that in favour of this last conclusion, at least in its general form, one can at present only give arguments, and not proofs, so that it is as well to separate it from the first twop ropositions, which appear to me to be definitely established.

REVIEWS.

I. — Description de la Faune Jurassique du Portugal. Embranchement des Échinodermes. Par P. de Loriol. Commission des Travaux Géologiques du Portugal. pp. 109-179; pls. xix.–xxix. (Lisbonne, 1890–91.)

THIS is the second and concluding part of M. de Loriol's admirable monograph on the Jurassic Echinoderms of Portugal. It first treats of the Irregular or Exocyclical Echinoids, of which 16 species are described, belonging to the genera *Holectypus*, *Pygaster*, *Pileus*, *Pyrina*, *Echinobrissus*, *Pygurus*, and *Collyrites*. Several of the species occur also in the corresponding Jurassic beds in this country. Of the Starfishes, only one species, *Aspidaster Delgadoi*, de Lor., has been met with, which is the type of the genus. The author considers that the well-known *Oreaster bulbiferus*, Forbes, from the Upper Chalk of this country, subsequently placed in an existing genus, *Pentaceros*, should rightly be included in *Aspidaster*. There are 34 species of Crinoids belonging to the following genera: *Dolichocrinus*, *Millericrinus*, *Pentacrinus*, *Balanocrinus*, *Extracrinus*, *Antedon*, and *Thollericrinus*. Some of the species are based merely on the characters of portions of the stems, more particularly amongst the Pentacrinoids, and in the genus *Balanocrinus* there are six species described wholly from joints of the stem; neither in Portugal nor elsewhere has the calyx of this genus been yet brought to light. In his description of *Eugeniacrinus caryophyllatus*, M. de Loriol points out the importance of Mr. Bather's observations on the presence of basals in a certain stage of the development of this form, which have evidently in the course of its growth been absorbed into the first radials; at the same time he thinks it correct to define the genus *Eugeniacrinus* as without basals, since in the mature stage of the animal, on which the generic characters are based, they are no longer present. In the family of the Eugeniacrinidæ, however, some genera, as *Trigonocrinus*, Bather, and *Dolichocrinus*, de Lor., have basals preserved in the calyx; but the author follows the late Dr. H. Carpenter in excluding from it *Eudesicrinus*, de Lor., which comes properly into the family Holopidæ. In another very peculiar form, *Dolichocrinus aberrans*, de Lor., likewise placed in the family Eugeniacrinidæ, basals were clearly present, and the first radials, five in number, are extremely long, and form by their union a cylindrical tube deeply furrowed in its interior.

In the first part of this monograph 95 species of Endocyclical Echinoids were described, thus bringing a total of 146 species of Echinoderms in the Jurassic strata of Portugal, of which 69 are new forms. This number is relatively small when compared with those known from the corresponding strata in France. The Lusitanian stage of M. Choffat, which comprises the higher horizons of the Jurassic strata in Portugal, is most prolific in Echinoderms. A notable feature is the enormous predominance of

the Endocyclical over the Exocyclical Echinoids in the Portuguese Jurassic.

The different forms are fully illustrated in the accompanying ten lithograph plates, and we may congratulate the Geological Survey of Portugal on the satisfactory manner in which it has published this monograph.

II.—Report on the Foraminifera dredged by H.M.S. "Gazelle," during the Years 1874–76. By Dr. J. G. Egger, etc., etc. With one Chart and 21 pages of figures. 4to. Munich, 1893. [Foraminiferen aus Meeresgrundproben, etc. Abhandl. k. bayer. Akad. Wiss., ii.Cl., xviii.Band, ii.Abtheil.]

NEARLY all the Foraminifera, from Nummulites downwards, are of interest to Geologists; for in many instances they go far to constitute the mass of rock; and often their particular forms, or the general facies of a series, characterize certain groups of strata. For instance, the special *Carboniferous* Endothyræ and Fusulinæ,—the poor breed of Nodosariæ and Cristellariæ in the *Lias* and *Oolites*,—the manifold arenaceous species in the *Jurassic* clays,—the well-grown Nodosariæ and Cristellariæ of the *Cretaceous* formations,—the distinctly-various *Lower-Tertiary* Nummulites,—the Polymorphinæ of the *Lower Crag*,—and the large Nodosariæ and Cristellariæ in the *Upper Tertiaries* of Italy. Some such groups are met with in existing seas; and other groups of living Foraminifera dominate here and there.

This fact is well shown in Dr. Egger's Report on the deep-sea Foraminifera obtained by H.M.S. "Gazelle," sent out by the German Government in 1874. The scientific results of this voyage were published in 5 vols. 4to. Berlin, 1888–90, excepting this Report, which has been communicated to and published by the Munich Academy of Sciences.

Precise economy has been practised in the production of text and plates. For the nomenclature and synonomy the reader is referred, for the most part, to Dr. H. B. Brady's Report on the Foraminifera dredged by H.M.S. "Challenger" (2 vols. 4to. London, 1884); and the multitudinous figures are massed in 21 pages of the text, having been copied and printed by the Meisener-Riffarth process. Thus the Porcellanous forms are closely grouped, as 189 species, in plates i. ii. and iii. at pages 26, 35, and 49; the Arenacea in plates iv. and v. with 98 species, at pages 60 and 67; Textulariæ in pls. vi. and vii. with 108 species, at pages 76 and 85; and so on with the Hyalina; Bulimina, Polymorphina, Lagena, Nodosaria, Cristellaria, Globigerina, etc., having separate pages, on which the crowded types and varieties may be studied at once, at pages 90, 114, 128, 146, 157, and 164. Eight more pages, of 411 figures, are devoted to the Rotalians and other interesting forms.

This wealth of illustrations, crowded into 21 pages, if not good as artistic plates, are yet very good and useful as galleries of portraits, bringing whole collections of types and their varieties—the heads of families and all their collateral relatives—under the eye at once and in close proximity to one another. T. R. J.

III.—Contributions à l'Étude de la Faune Jurassique de Normandie. Premier Mémoire sur les Trigonies. Par A. Bigot. Mémoires de la Société Linnéenne de Normandie, xviie volume, 2e fascicule. 4to. pp. 261–345, pls. viii–xvii. (Caen, 1893.)

THE collection of fossils belonging to the Faculté des Sciences of Caen comprises a large number of *Trigonias*, which have been mostly obtained from the same beds and localities of the Jurassic series in Normandy which furnished the type specimens described by Agassiz and by D'Orbigny. The late E. E. Deslongchamps made a preliminary study of these forms, without, however, leaving behind more than a few notices respecting them; his successor, Prof. Bigot, has undertaken their investigation afresh, at the same time retaining the MS. names which Deslongchamps had bestowed upon the new species. This Memoir treats of the forms from the Inferior Oolite, the Great Oolite, and the Oxford and Kimmeridge Clays of Normandy, but it does not include those from the Lias of the same region, nor the Portland species from Bray, which latter have, moreover, been carefully studied by Munier Chalmas, de Loriol and Pellat. In all, Prof. Bigot here describes about fifty species, amongst which twenty-one are new.

In his introductory remarks, the author considers that the section "Glabræ" of Agassiz might be suitably divided into three; a grouping already in part suggested by Lycett. The first of these, "Semi-læves," includes *T. Lingonensis*, Dum., *T. Beesleyana*, Lycett, and *T. Eudesi*, n.sp., from the Lias, Bajocien and Bathonien respectively. The second, "Gibbosæ," has for its principal species *T. gibbosa*, Lycett, *T. Manselli*, Lycett, and *T. Damoniana*, from the English Portland Beds, and five other species from the Upper Jura of France. In the third section, "Excentricæ," are ranged species which commence in the Upper Jura and continue into the Cretaceous; the typical species are, *T. excentrica*, Sow., *T. læviuscula*, Lycett, and *T. Boloniensis*, de Loriol.

The detailed descriptions and excellent figures which Prof. Bigot has given of these Normandy forms are of particular interest to English students of *Trigonias*, in view of the fact that the types of more than one-third the number of the species here described belong to the Jurassic rocks of this country, and the author's review of the descriptions of Lycett, Morris and Sowerby deserves the most careful consideration. G. J. H.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—March 7th, 1894.—Dr. Henry Woodward, F.R.S., President, in the Chair. The following communications were read:—

1. "The Systematic Position of the Trilobites." By H. M. Bernard, Esq., M.A., F.L.S., F.Z.S. (Communicated by Dr. Henry Woodward, F.R.S., P.G.S.)

The author, in his work on "The Apodidæ," endeavoured to show that the *Apus* was the ancestral form of all existing Crustacea except the Ostracoda, and as such might be expected to throw light upon the Trilobites. Since the publication of this work he has been studying the organization of the trilobites themselves, and the results are given in the present communication. He discusses the great variability in the number of segments shown by the trilobites; the formation of the head by the gradual incorporation of trunk-segments; the bending round ventrally of the first segment; the "wandering" of the eyes; the existence and modification of the "dorsal organ;" and especially the character of the limbs.

As a result of this discussion, he states that the zoological position of the trilobites can now be fixed with considerable probability. The features described serve to connect the trilobites with *Apus*. *Apus* must be assumed to lie low in the direct line up from the original annelidan ancestor towards the modern crustacea, and the trilobites must have branched off laterally from this line, either once or more than once, in times anterior to the primitive *Apus*, as forms specialized for creeping under the protection of a hard imbricated carapace, obtained by the repetition on every segment of the pleuræ of the head-segments, which together form the head-shield.

The trilobites may be briefly described as fixed specialized stages in the evolution of the crustacea from an annelidan ancestor with its mouth bent round ventrally, so as to use its parapodia as jaws.

2. "Landscape Marble." By Beeby Thompson, Esq., F.G.S., F.C.S.

The Cotham Stone is a hard, close-grained, argillaceous limestone with conchoidal fracture. The dark arborescent markings of the stone rise from a more or less stratified dark base, spread out as they rise, and terminate upwards in wavy banded portions of the limestones. In some specimens two "landscapes" are seen, one above the other, each rising from a distinct dark layer.

The author describes the microscopical and chemical characters of the rock, and its mode of occurrence, and discusses the explanations which have been put forward to account for its formation, especially that of Edward Owen, who in 1754 gave the first published description of the Cotham Stone, and that advanced by Mr. H. B. Woodward in the Geological Magazine for 1892. He then proposes a new explanation to account for the formation of the rock, and maintains that its peculiar characters are due to interbedded layers of vegetable matter, which decomposed and evolved carbonic-acid gas and marsh-gas. This decomposition continued while several inches of new sediment were laid down, the result being that arborescent markings were produced along the lines taken by the escaping bubbles, and that the upward pressure of these gases, after their escape had been prevented by increasing coherence or greater thickness of the upper layers of sediment, caused the corrugations in the upper surface of the stone. He further discusses the composition of the stone, and describes experiments which he made to illustrate his views.

3. "On the Discovery of Molluscs in the Upper Keuper at Shrewley, in Warwickshire." By the Rev. P. B. Brodie, M.A., F.G.S.

Mr. R. B. Newton read a paper at the meeting of the British Association at Nottingham in 1893, on some Lamellibranchs found at Shrewley by the author of the present paper and Mr. Richards. In this paper details of the section where the shells were found are given, and their interest and importance pointed out, no shells having been previously detected anywhere in the New Red Sandstone in this country.

II.—March 21st, 1894.—Dr. Henry Woodward, F.R.S., President, in the Chair.

The President gave expression to the feelings of regret with which the Society had received the announcement of the death of Mr. William Pengelly, F.R.S., of Torquay. He had been a Fellow since 1850, and had taken a leading part in the exploration of Kent's Cavern, Torquay, and Brixham Cave.[1]

The following communications were read:—

1. "On the Origin of certain Novaculites and Quartzites." By Frank Rutley, Esq., F.G.S., Lecturer on Mineralogy in the Royal College of Science, London.

The novaculites of Arkansas have already been admirably described by Mr. Griswold in vol. iii. of the Arkansas Survey Report for 1890. One of the characteristic microscopic features in Ouachita stone is there stated to consist in the presence of numerous cavities, often of sharply-defined rhombohedral form, which Mr. Griswold considers to have been originally occupied by crystals of calcite or dolomite.

The author, while admitting that the cavities were no doubt once filled by the latter mineral, ventures to differ from Mr. Griswold, and some of the authorities he cites, concerning the origin of the rock. Crystalline dolomites, when dissolving, become disintegrated into minute but well-formed rhombohedra. As the process of dissolution proceeds these crystals may become so eroded that the rhombohedral form is no longer to be recognized. The author points out that no inconsiderable proportion of the cavities in Ouachita stone present irregular boundaries, such as the moulds of partially eroded rhombohedra would show. He then offers a fresh interpretation of these cavities, so far as the origin of the rock is concerned:—

1st. He assumes that beds of crystalline magnesian limestone have been slowly dissolved by ordinary atmospheric agency and the percolation of water charged with carbonic acid or other solvent.

2nd. That, as the limestone was being dissolved, it was at the same time being replaced by silica, which enveloped minute isolated crystals and groups of crystals, some perfect, others in various stages of erosion.

3rd. That the silica assumed the condition of chalcedony, its specific gravity, as stated by Mr. Griswold and as determined by the author, being low in comparison with that of quartz.

[1] See Obituary, p. 238.

4th. The residuum of the original dolomite or dolomitic limestone was removed, leaving the perfect and imperfect rhombohedral cavities.

A calciferous, gold-bearing quartzite from the Zululand gold-fields is described and a similar origin is ascribed to it, but in this case the original rock appears to have been simply a limestone, not a dolomite. The gold seems to occur chiefly in the calcareous portions of the rock. The author has also been tempted to suggest a similar origin for the saddle-reefs of the Bendigo gold-field. In all of these cases the train of reasoning is based upon the conclusions arrived at in his previous paper "On the Dwindling and Disappearance of Limestones." He indicates that the stratigraphical relations of the Arkansas novaculites, as described in Mr. Griswold's Report, are such as to warrant the assumption that limestones once occurred in the position now occupied by beds of novaculite. Many collateral matters are dealt with in the paper which cannot be given in abstract; among them is an attempt to classify quartzites.

2. "Note on the Occurrence of Perlitic Cracks in Quartz." By W. W. Watts, Esq., M.A., F.G.S.

The author of this communication describes some specimens of the porphyritic pitchstone of Sandy Braes in Antrim, which are deposited in the Museum of Science and Art in Dublin, and in that of Practical Geology in Jermyn Street. They exhibit admirable examples of perlitic structure in the brown glassy matrix, and the presence of polygonal, circumferential, and radial cracks is noticed. The porphyritic crystals of quartz are traversed by curved fissures of retreat, not so perfect as those found in the glass, but better than those usually produced by the rapid cooling of Canada balsam. The fissures in the quartz are frequently prolonged into the matrix, undergoing only a very slight and almost imperceptible deviation in direction at the junction. But in addition to this the quartz is often found to act as a centre of strain, the inner cracks of the perlite being wholly in quartz, the next traversing both, and the outer ones in glass only. In other examples the outer cracks of a matrix perlite sometimes enter the quartz, while in others polygonal cracks occur, and join up in, the quartz and give off radial cracks precisely like those of the matrix. These observations lead to the conclusion that the quartz and glass must have contracted at about the same rate, and that the observation of perlitic structure in a rock with trachytic or felsitic matrix by no means proves that the rock is necessarily a devitrified glass. References are given to somewhat similar observations by Fouqué and Michel-Lévy, and by Iddings.

III.—April 11th, 1894.—Dr. Henry Woodward, F.R.S., President, in the Chair. The following communications were read:—

1. "Mesozoic Rocks and Crystalline Schists in the Lepontine Alps." By T. G. Bonney, D.Sc., LL.D., F.R.S., F.G.S., Professor of Geology in University College, London, and Fellow of St. John's College, Cambridge.

The author described the results of an examination of the infold of Jurassic rock in the Urserenthal, undertaken in the hope of finding some definite evidence as to the relations of the marble exposed near the old church at Altkirche, and the adjacent Jurassic rocks. Good sections are few and far between; for the comparatively perishable nature of the rock causes it usually to be masked by débris and turf.

The easternmost of the sections described occurs high up on the slopes north of the Oberald road. Read off from the northern side it exhibits (1) gneiss, (2) phyllites with bands of subcrystalline limestone, etc.—Jurassic, (3) a little rauchwacke, (4) "sericitic" gneiss. The next section (about 250 feet above the St. Gothard road at Altkirche) gives (1) gneiss, (2) covered ground, (3) slabby marble, (4) phyllite, (5) thicker mass of slabby marble, (6) phyllite, etc., (7) "sericitic" gneiss. The third section (just above the church) runs thus, using numbers to correspond with the last: (1) gneiss, (4) phyllite, (5) slabby marble, (6) phyllite, etc., (7) "sericitic" gneiss. It must be remembered that on the slopes of the Oberalp farther south, between the "sericitic" gneiss and the "Hospenthal schists," another dark phyllite is found, generally considered by the Swiss geologists to be Carboniferous. The marble in the third section is in places distinctly banded with white mica, and passes on the northern side into fairly normal mica-schist and quartzose-schist. The fourth section, about a mile away, on the left bank of the Reuss valley, gives a practically continuous section in phyllite and dark limestone, without any marble. In the fifth section, rather more than a mile farther, if any marble be present, it is very thin and shattered. At Realp, about $3\frac{3}{4}$ miles farther, the next good section is obtained. Here the rocks go in the following order (from the northern side): (1) gneiss, (2) phyllite and limestone, (3) subcrystalline limestone, looking very crushed, (4) the marble, (5) phyllite, etc., (6) Hospenthal schists. The last group of sections occurs near the Furka Pass. In the first, crossed by the high road, there is no marble, but a little rauchwacke on the southern side. The next one, on the slopes below the pass, seems to show two masses of the marble parted by a subcrystalline limestone like that at Realp, with phyllite above and below. Of the two masses of marble, the southern one can be traced right across the Pass, but the extent of the other is not so clear.

Examination of the marble mentioned above shows in all cases that it has been considerably modified by pressure since it became a crystalline rock. The author discusses the evidence of these sections, and maintains that the hypothesis that the marble is an older rock intercalated by thrust-faulting among Jurassic strata leads to fewer difficulties than to consider it as belonging to the same system.

In the latter part of the paper the results of a re-examination of the ravine-section in the Val Canaria, and of some studies of the south side of the Val Bedretto, are described, which, as the author maintains, confirm the view already expressed by him, viz. that the schists with black garnets, mica, kyanite, dolomite, and calcite

(the last sometimes becoming marbles) are not altered Jurassic rocks, but are much older.

2. "Notes on some Trachytes, Metamorphosed Tuffs, and other Rocks of Igneous Origin, on the Western Flank of Dartmoor." By Lieutenant-General C. A. McMahon, F.G.S.

In this paper the author notices the occurrence of felsite and trachyte at Sourton Tor; of rhyolite and of aluminous serpentine at Was Tor; and of a dolerite at Brent Tor in the exact situation indicated by Mr. Rutley as the probable position of the throat of the Brent Tor volcano.

The author describes extensive beds of tuffs at Sourton Tor and Meldon, the matrix of which has been converted, by contact-metamorphism, into what closely resembles the base of a rhyolite, and which, in extreme cases, exhibits fluxion-structure, or a structure closely resembling it. The fragments included in this base are so numerous that six or seven different species of lavas may be seen in a single slide; this fact, and a consideration of the extensive area over which these beds extend, lead the author to believe that these beds are metamorphosed tuffs and not tufaceous lavas.

He then describes some beds on the flank of Cock's Tor, which give evidence on their weathered surface of an original laminated structure by exhibiting a corded appearance like corduroy cloth. These beds are composed of colourless augite, set in a base which in ordinary light looks like a structureless glass, but which between crossed nicols is seen to be an obscurely crystalline felspar.

The author compares these rocks with that portion of the Lizard hornblende schists for which a tufaceous origin was proposed by De la Beche and other writers, including Prof. Bonney and himself. He shows that the Lizard schists and the Cock's Tor rocks agree in specific gravity and in some other characteristics; and he concludes that at Cock's Tor the first stage in the conversion by contact-action of beds of fine volcanic ash into hornblende-schist had been completed, and the final stage, due to aqueous agencies, had just begun.

The paper concludes with some remarks on the relationship of the epidiorites to the rocks of volcanic origin.

CORRESPONDENCE.

GEOLOGICAL DIFFICULTIES IN THE LEPONTINE ALPS.

Sir,—Dr. Stapff's interesting paper in your April Number seems to call for a few words of explanation from myself. As regards the Altkirche marble, the problem which it presents has been discussed in a paper recently read before the Geological Society, to which I may refer. Here, it may suffice to say, that I have not denied that certain limestones near Altkirche may contain fragments of organisms, but doubt the occurrence of these in the true marble, *i.e.* we have here not only to distinguish imitative from truly organic markings (in which photographic evidence often is not sufficient), but also to settle the age of the rock in which the latter occur.

As for the Piora schists, I am sorry to have misunderstood Dr. Stapff, but think he would have done better to refer to my letter as published in the GEOLOGICAL MAGAZINE (1892, p. 90) instead of to an abstract of it, necessarily condensed. He will find my words to be "If I am right in understanding Dr. Stapff to assign the Piora schists to the Carboniferous system," etc. The fact is that I had great difficulty in coming to a conclusion both as to the exact position of the divisions which he had drawn in his published sections and paper, and as to how much was covered by the terms which he employed. The terminology of petrologists at present is rather unsettled, so that we do not seldom find difficulties of this kind arising in regard to details. But if the Piora schists are part of a series extending "from the Carboniferous to the Jurassic age," I fear that I must leave the remainder of the sentence partly quoted at the top of page 160 otherwise unaltered.

My remark as to the inadequacy of photographs to decide whether organisms occurred in the Altkirche marble applies equally to the "sand grains" in the Guspis gneiss. That a gneiss may be of sedimentary origin I do not deny, but I doubt whether it would be possible to recognize with certainty the original clastic grains, unless they had been so large as to make this term inappropriate. For instance, I have examined many quartz-schists, in which I suspected certain grains to indicate the position of original constituents, but have met with only one case which I felt would satisfy a sceptic (discovered by Mr. J. Eccles last summer), and here they were pebbles rather than grains. But I have seen many cases where a structure, due to the crushing of a crystalline rock, wonderfully simulates that of an ordinary clastic rock, so, as I have been more than once led into error in this matter, the proverb holds good, "once bit, twice shy."

T. G. BONNEY.

DR. ALEX. BROWN ON *SOLENOPORA*.

SIR,—In Dr. Brown's article on the structure of *Solenopora* there is a slight error in the horizon given for the Yorkshire specimens which should be corrected, especially as the rock in question was for some time considered to be the equivalent of the Great Oolite. The Malton specimens are obtained from the Corallian, and they are also very abundant throughout the Ayton-Brompton Coral Rag.

GEOLOGICAL SURVEY,
LEICESTER.

C. FOX STRANGWAYS.

MR. WATTS'S PAPER ON THE TARDREE PERLITE.

SIR,—On thinking over the subject of Mr. Watts's interesting paper read before the Geological Society on March 21st last, in which he endeavoured to prove—by means of very beautiful magnified sections of the Tardree trachyte—that the perlitic structure is sometimes continued from the glassy magma into the enclosed crystals of quartz, it seems to me that the essentially distinct molecular structure of the two mineral substances was not sufficiently taken into account, and that it is only on a recognition

of these structures that an explanation of the phenomena can be found. I quite admit that some of Mr. Watts's sections showed that the perlitic shrinkage fissures of the glass-magma did pass into, and through, the quartz-crystals; but it is not to be inferred from this that the quartz-crystals had perlitic structure, as I shall endeavour to show. Let us consider for a moment the process of consolidation from the molten state. It is clear that the silica consolidated before the glass-magma; and in forming distinct crystals it obeyed the law of its molecular constitution, which obliges it to take the form of a hexagonal prism terminated by a pyramid. On the other hand the amorphous (or non-crystalline) magma is governed by an entirely different set of molecular forces and tries to form a series of concentric globules, somewhat as basalt on cooling often forms globular masses with concentric envelopes. The globular is the primary form of a cooling body. It is clear that the molecular constitutions of the quartz-crystal and of the non-crystalline magma are essentially different, and this distinction finds its result in the diverse forms and structures of the two. If this be admitted it will not be difficult to account for the apparent perlitic fissures traversing some of the quartz-crystals, as shown in some of the specimens. Though these have had priority over the magma in consolidation, yet they were necessarily highly heated and somewhat soft owing to the liquid state of the enclosing magma; and this being so, as the perlitic fissures were being developed in the latter they would exert a pull upon the parts of the quartz-crystals in contact, the force developed by contraction in the case of each globule of the magma tending to draw the mass towards its centre. The quartz being (as has been observed) in a somewhat soft condition would be unable to withstand this force, and in such cases it would give way; and fissures would be produced continuous with one or more in the enclosing glass-magma; bnt this does not prove that the quartz itself has a perlitic structure.

EDWARD HULL.

22*nd March*, 1894.

ON THE POSSIBLE MARINE ORIGIN OF THE LOESS.

SIR,—One of the difficulties that faced me in my paper on the "Submergence of Western Europe"[1] was the want of evidence to prove distinctly that the land had been under the sea. As, however, all the physical evidence concurred to show that the various forms of the Rubble-drift indicated a transient disturbance, I concluded that the submergence had been of too short a duration to allow of the establishment of a marine fauna in the area submerged. It was therefore with much satisfaction that I found the other day confirmation of a very unexpected character in a paper published in the last number of the "Bulletin de la Société Belge de Geologie."[2] In treating of the Loess I described a portion of it as of fluviatile origin,

[1] Phil. Trans. for 1893, p. 903.
[2] *Ibid.* March 1894, p. 118.

whilst I considered that the high- and mid-level Loess belonged to the Rubble-drift, and was therefore of marine origin. In the paper I now refer to, by M. Xavier Stainor, he gives the analyses of some of the Belgian Loess, in which, besides the ordinary ingredients, a notable proportion of Chloride of Sodium, in one case as much as 1·17 per cent., has been found. This Loess contains the usual common land shells—*Pupa marginata, Succinea oblonga, and Helix hispida.*

JOSEPH PRESTWICH.

SHOREHAM, KENT.
April 19th.

OBITUARY.

WILLIAM PENGELLY, F.R.S., F.G.S.

BORN 12TH JANUARY, 1812. DIED 17TH MARCH, 1894.

THERE has just been gathered to his rest, in his 83rd year, one of that small band of Geologists who assisted Falconer, Busk, Lyell, Prestwich, Lartet, Christy, Evans, Rupert Jones, Boyd Dawkins, and a few others, to place upon a scientific basis that inquiry into the evidence of Pre-historic man which was systematically commenced in this country by the exploration of Brixham Cave in 1858. This work, which was carried out under the auspices of the Royal and Geological Societies by Mr. William Pengelly, of Torquay, yielded most important results, and was followed, in 1864, by a similar investigation of the historic Kent's Cavern, Torquay (originally partially explored by the Rev. J. McEnery in 1825), and, like the Brixham Cave, carried out with untiring zeal by Mr. Pengelly for a period of more than fifteen years (see British Association Reports 1865–1880).

WILLIAM PENGELLY was born at East Looe in Cornwall, January 12th, 1812. Coming to Torquay as a young man, he opened a school, which he carried on for some years on the Pestalozzian system, and was one of the first to introduce the use of the chalk and black-board in imparting instruction.[1]

The author of numerous treatises on the Devonian and Triassic Rocks of Devonshire, on St. Michael's Mount in Cornwall, and many other geological subjects, Mr. Pengelly, in conjunction with Dr. Heer, of Zurich, published a monograph on "The Lignites of Bovey Tracey," which is regarded as a most valuable scientific work. His assiduity in the collection and arrangement of specimens is testified by the magnificent series of Devonian fossils which, under the title of the Pengelly Collection, was lodged in the Museum of Oxford University by the Baroness Burdett-Coutts in connection with the Burdett-Coutts Scholarship, and also by the splendid collections of bones and flint-implements from Kent's Cavern, which he has placed in the British Museum and in the Museum

[1] Amongst his private pupils in Mathematics was Miss (afterwards the Baroness) Burdett-Coutts, who, through life, remained his staunch friend.

of the Torquay Natural History Society. Ever eager to foster a love for science among the people, and to encourage studies which had for their object the advancement of scientific knowledge, Mr. Pengelly was especially active as a lecturer, and could easily make himself understood by persons totally ignorant of science. Twenty years ago he travelled through the country as one of the most acceptable lecturers of the day. His efforts locally were equally successful. He was instrumental in 1837 in establishing the Torquay Mechanics' Institute, and seven years later, together with the late Mr. Vivian and others, he originated the Torquay Natural History Society, of which he was the Honorary Secretary for many years, and is the last of the original founders. In 1867 he started the Devonshire Association for the Advancement of Science, Literature, and Art, of which he was the President in 1867–8. From their inception he took a prominent part in these institutions, and his eminent services in many capacities have been recognized on several occasions by the members. Undoubtedly Mr. Pengelly was an enthusiast. As he said in his Manchester lecture in 1872, "the pleasure of the work is the payment," but his valuable and historic labours have not been unrecognized. A regular attendant at the annual meetings of the British Association, over the Geological Section of which he was President, and which for many years he served as Secretary. He was elected F.G.S. in 1850, and received the distinction of F.R.S. in 1863. In 1874 he was the recipient of a testimonial of between 500 and 600 guineas "in recognition of his long and valued services to science generally, and more especially for the exploration of Kent's Cavern, Torquay."

In 1877 Mr. Pengelly was awarded the "Lyell Geological Fund" by the Council of the Geological Society, and in 1886 the same body conferred upon him the "Lyell Medal" as a mark of honorary distinction, and as an expression of the Council's recognition of his valuable scientific labours.

Mr. Pengelly was presented with his portrait, painted by A. S. Cope, which now adorns the library of the Natural History Society's Museum in Babbacombe-road, Torquay. On retiring from the Secretaryship of the Society in 1890 he received an address from the members, who "desire to record the high appreciation of the invaluable services rendered to the Society by Mr. Pengelly, who for thirty-nine years has fulfilled the onerous duties of Honorary Secretary," at the same time expressing "profound regret at his retirement." Altogether the time spent in the exploration of Kent's Cavern was over fifteen years—from 28th March, 1865, to 19th June, 1880—the cost being met by the British Association and various scientific friends. Mr. Pengelly wrote no less than ninety-two separate memoirs. He was a member of the Society of Friends, but it is as a scientific man that Mr. Pengelly will be best known and remembered. Mr. Pengelly was twice married, but all his children by his first wife died; the last, Alfred Pengelly, being killed in India whilst hunting big game. He leaves a widow and two daughters, who share their father's scientific tastes.

JOSEPH BICKERTON MORGAN, F.G.S.

BORN 1859. DIED MARCH 8TH, 1894.

On the 8th of March there passed away at Ventnor a life full of promise for work among the Ordovician and Silurian rocks. Born in 1859, Mr. Morgan was always an earnest student of nature, and one of his earliest tasks, when associated with the late Morris Charles Jones as Assistant Honorary Curator of the Powysland Museum at Welshpool, was to classify and arrange the large collection of recent shells given to the Museum by the Rev. J. Vize. He next turned his attention to the fossils in the Museum, rearranged them, and then added very largely to them from his own extensive collections, especially from the Silurian and Ordovician rocks in the immediate neighbourhood.

In 1885 a paper, published by Mr. Vine in the Quarterly Journal of the Geological Society, contained a description of a new species of *Phyllopora*, *P. tumida*, from the Caradoc rocks of Llanfyllin, and of *Thamniscus antiquus*, from the Bala volcanic ash of Middletown Hill, both collected by Mr. Morgan. Previously to this Mr. Morgan had been studying the Upper Ordovician and Silurian rocks of Powysland and the Welsh border, and had obtained a large series of fossils from beds above and below the boundary line of these formations, which he submitted to Professor Lapworth for identification. By the advice of the latter he began to map these strata, and succeeded in defining the lower limit of the Silurians with much greater exactitude than had been hitherto done, with the result of adding several hundreds of feet of rock to the Silurian system. A forecast of his conclusions on the subject was published as a paper to the British Association at Leeds in 1890, but the full paper has not yet been published. It is to be hoped that sufficient material may be found amongst his papers to allow of its publication in full.

In 1892 Mr. Morgan resolved to devote himself to scientific work, and, obtaining a free scholarship at the Royal College of Science, he came to London, and in a single year succeeded so well as to obtain the first prize at the College, together with the Murchison medal and gift of instruments and books. It was on the last day of his duties as demonstrator during the summer vacation that he took a chill, from which serious trouble ensued, compelling him to abandon work and winter at Ventnor. Here he seemed to rally, and just as his friends were beginning to hope that he might soon be well enough to resume his work, he succumbed to an attack of hæmorrhage, in the 35th year of his age. In addition to the work mentioned above, some new species of *Entomostraca* were discovered by him, while he contributed several papers, including one on Shorthorn Cattle, and one "on the Strata forming the base of the Silurian in north-east Montgomeryshire," to the Montgomery Collections, and delivered lectures at the Welshpool School of Art.

Thus a country geologically almost unknown has lost an earnest, reliable, and enthusiastic worker, one who could very ill be spared; while many of us have to mourn a kind and true-hearted friend.